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CHILD BEHAVIOR

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CHILD BEHAVIOR

A CRITICAL AND EXPERIMENTAL STUDY OF YOUNG CHILDREN BY THE METHOD OF CONDITIONED REFLEXES

BY

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TO

WILLIAM H. BURNHAM

FROM WHOSE LECTURES ON KRASNOGORSKI THE INSPIRATION
FOR THIS STUDY WAS DRAWN AND TO WHOSE KINDLY
HELP ITS COMPLETION IS DUE

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INTRODUCTION

"Child Study" is a term so well-worn and so widely used that its application and meaning in any one instance consequently needs definition. Studies are made of the growth of the child, his games, his vocabulary, his mental activities, and they are all legitimate branches of child study, somewhat overlapping, it is true, and all of them far more fragmentary than complete.

One of the lines along which a great deal of work has been done is that of the mental activities of the child or on what might justly be called child psychology. This division is recognized from the standpoint of psychology itself and is again given varied connotations as it deals with different features of the subject. All of these subdivisions have, however, one thing in common. They treat of the growing organism as contrasted with the predominantly functioning organism of the adult. Bearing this characteristic in mind, child psychology may be justly said to include the study of all the stages of the intellectual development of the organism previous to the stage of complete maturity of mind. Usage has rather clearly discriminated for us, however, three main divisions in this long period of development. These are usually studied separately, although not without relating the findings of any one period to those of the others.

The most widely known and best developed of these three divisions of child psychology is educational psy-

chology or psychology as it applies to the child of school age and consequently to his education. Partly overlapping this but separated from it by the rift of enormous physiological changes is the psychology of the adolescent. In its turn the study of the mind of the adolescent can be separated from the study of the mind of the adult only by rather artificial discriminations.

On the other extreme of the age-limits of childhood we have the child of the pre-school age. Here the term "Child Study" is generally used whether or no the problem attacked is a psychological one. In view of the tremendous development of intellect that occurs in this period and considering the large number of studies of such development that have already been made, it seems logical to use the term "Psychology" in distinguishing these studies from investigations dealing with physical development, social reactions, and sense functioning, despite the fact that there are necessary correlations which must be considered.

But psychology must itself be defined. Shall the term be used to designate any type of study which describes the child's mental processes through observation, analogy, interpretation, and experimentation or shall the usage be more limited? In so far as one wishes to be scientifically accurate and in accord with the accepted meaning of to-day the term should be used only to indicate the report of observations made under controlled conditions which are such that the experiments may be repeated and the results verified by any competent person at any future time.

Even so the term "Psychology" has a connotation which is very apt to be misleading when applied to the study of young children. Psychology deals with mental

processes, with images, ideas and judgments. These the young child can not report to us, nor can we study them directly in their functioning in him, but only through inference, deduction and analogy. Results obtained thus are not scientific in the sense of the natural sciences. The animal psychologists have met and conquered this same situation and now study the behavior of their subjects without dependence upon the assumption of such and such subjective processes. They are content to ascertain the exact relations existing between stimulus and reaction. It seems fairly logical that those wishing to enroll the study of the young child among the sciences should take the same step, should be content to study his behavior. Of course in its wider meaning this would include all the activities of the child but in a stricter sense we may use it to mean the study of more or less complex reaction to definitely ascertained stimuli.

The term "Child Behavior" will therefore be used in this discussion to indicate the observations of and the experimentations upon the mental processes of children under school age, as expressed in their behavior or reaction to stimuli and ascertained under conditions permitting repetition and verification. But the value of any such study will be greatly increased if several additional factors are taken into consideration. The problem on learning, memory, language, or anything else will be far more valuable if it is studied in connection with full recognition and evaluation of the physical organism, the environmental conditions, and the past experiences of the child. Also, the problem undertaken, whatever it is, should be correlated with and compared with similar studies on older children.

Moreover, if we are attempting to formulate a basis

for a science of Child Behavior, there are a few questions we should ask before undertaking any specific investigation. The answers to these questions will undoubtedly indicate the most probable lines of profitable work. The questions are the following. What work has been done and what results have been obtained regarding the mental development of the young child? To what extent do these investigations fail and what are the reasons for their errors? What improvements in technique, apparatus and method can be made in the new work to be done?

Then, when a piece of research has been completed, the results should be evaluated not only from the standpoint of the absolute findings but also from the standpoint of their interpretation, their significance for science in general and, in this age of applied values, from the standpoint of their use. Also, every problem should give, as a result of its being intimately known through experience, suggestions for modifications of use in future experiments and also suggestions for new but correlated investigations.

Let us here attack these problems in the order mentioned, trying to explain more fully by example just what is meant. First, in order to avoid duplication and to avail ourselves of the best that the experience of past experimenters has to offer we must survey the results so far obtained by those studying the young child. These results will be found in the various books on "Child Study."

CHILD BEHAVIOR

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CHAPTER I

HISTORICAL SURVEY OF CHILD STUDY

IT would be interesting to trace the evolution of the place the child has played in human thought and plans since the earliest days of recorded events but that would lie a little aside from the main purpose of this discussion.

All we can do here is indicate the more important factors in the development of child study and see how they give rise to child psychology as a separate line of research and theory.

The place of the child was ideal in the days of Hellas; statesmen, philosophers and citizens in general were devoted to his interests and gave of their best to his training. To them this appeared as but natural since he was the future State and only through giving him the most perfect preparation would the future State be as mighty as possible. Schools were pleasant places and the lessons of the Greek boy would seem play to the present-day child, but one great principle appears to have been disregarded. The boy was trained as a part of the group, the goal to be reached subserved the purpose of the group and the child as an individual was unrecognized.

With Christianity the child sank into obscurity. The same faith which elevated one child to Divinity made all other children sinful in their very nature and the Dark Ages could from the standpoint of the child have had no other cognomen. The stress was everywhere other-worldly, although this gradually changed as the Renaissance dawned and glimpses of brighter things may be seen.

The first ray of light which we can point out objectively as indicating the advent of a better day is the appearance of the Orbis Pictis of Comenius in 1658. This first picture book for children shows us several things. The child is coming once more into the focus of attention and he is regarded as a creature having some interests peculiar to himself which must be met in a way adapted to him. The stern necessity of making him an honorable and righteous man is now seen to be not incompatible with making him more happy as a child. Perhaps we might appear to be deducing too much from the advent of this book were it not that the same spirit is reflected throughout all the rest of the writings of Comenius, while the little book's immediate use and popularity throughout Europe seem to indicate that the people were ready for it. Nevertheless, another hundred years elapses before we find a generalized and definite expression of the rights of the child. This we owe to Rousseau.

With the appearance of the Emile in 1762 we have the beginnings of real child-study, both theoretical, educational and observational. We can do no better than let the author speak for himself. In the preface to the Emile he writes: "It (the Emile) was begun to give pleasure to a good mother who thinks for herself . . . We know nothing of childhood; and with

our mistaken notions the further we advance the further we go astray. The wisest writers devote themselves to what a man ought to know, without asking what a child is capable of learning. They are always looking for the man in the child, without considering what he is before he becomes a man. It is to this study that I have chiefly devoted myself. . . . I may be greatly mistaken as to what ought to be done, but I think I have clearly perceived the material which is to be worked upon." (145, p. 1.)

Rousseau's estimation of his own work may be accepted to-day. Education according to his plan, eliminating the natural social relations and demanding a laborious trial and error method of finding knowledge, may appear very far from ideal but the demand that the child be allowed free activity from the time of birth, with careful consideration and individual study; the insistence upon the fact that the child has certain rights of his own; the keen observations of child nature; all show that he indeed had grasped the significant features of the situation and recognized in the study of the child himself the solution of the problems of the child and of the future man. We might trace his influence upon human thought from that day to this but we must here narrow our study to tracing only the written records of the more influential students of the child since the time of Rousseau.

The earliest records of observations of the development of young children are probably those made by Pestalozzi (134) during the year 1774 upon his little son who was then just three and a half years old. They are not primarily psychological but a homely diary of the father's attempts at educating the child, interspersed with keen observations regarding his develop-

ment mentally. It is significant that although Pestalozzi thought too little of it to publish it, this fragment of observations contains much that would be considered praiseworthy to-day. It is the humane, personalized document of a man who wrote not as an abstract scientist but as one who knew people and who loved children. But, just as Pestalozzi was interested primarily in the tendencies and abilities of children in general in order to better formulate, upon them as a basis, his principles of education, so here, also, the observations in themselves are only a means to an end. We must not forget that to Pestalozzi the child was merely the immature and future man. To quote Heubaum, "Pestalozzi had not grasped the truth that the age of childhood had significance and value in and for itself, that it carries its end and purpose in itself" (78, p. 366).

And so Dietrich Tiedemann (174) is the real founder of child study. His observations of the development of his son appeared in 1787. They are purely psychological and reflect the empirical attitude of the times. Tiedemann deplores the fact that there are not more studies of these early manifestations of the development of the mental abilities because through such studies that part of the theory of the soul which studies the development of the various mental abilities in man would be considerably advanced and hence it would further pedagogy which is necessarily based upon it. The study, despite its excellence, attracted very little attention, sank into oblivion, and was not rescued until Michelan published a French translation of it in the *Journal général de l'instruction publique* in 1863.

Tiedemann's "Die vier erste Jahre meiner Kinder" (175) does not seem to have been any more influential

until it, also, was re-published in the same journal, the same year.

Froebel's "Die Menschenerziehung" (49) appeared in 1826. This, again, is the work of one who studies the young child motivated by the desire to obtain a factual basis for the development of a system of education. His observations are a mixture of empiricism and philosophical inferences. One new step he takes is of special significance from our standpoint. He points out that the child, the boy, the youth are not distinct stages but that they are continuous and that transitions are unbroken. Hence he speaks of "man in the period of earliest childhood."

In the same year Schleiermacher was expressing his views on childhood and education in his lectures at the University of Berlin. To him we owe the first complete formulation of the modern or Rousselian attitude towards the child which, in spite of the influence that his lectures must have had, develops very slowly aside from the secondary consideration that it receives in educational theory. Too much credit can hardly be given to the man who formulated the theory that "just as each period of life is for itself a natural division of life and has its own definite character: so, also, should each of these periods be enjoyed in its own specific character and not be regarded simply as a means to a later period" (147, p. 317).

The immediate followers of Schleiermacher do not seem to profit by, nor to even be cognizant of, the immense advance which his attitude indicates.

In 1828 Mme. Necker de Saussure published the first volume of her "Education Progressive" (121) in which she treats of the first four years of life. The second volume appeared in 1832 and carries the child beyond

the age with which we are concerned here. Mme. Necker's observations of the development of mental abilities in young children are extremely fascinating, being filled with the spirit of one who wrote with an understanding of her subject matter, and they deserve wider recognition than they have received in this country. But, although the observations she records are fairly accurate, they are not made from the standpoint of a study of the child for the child's sake. Her self-confessed motive is a book on Christian education, the "constantly progressing education," not completed in this life. Her study of the child is primarily to obtain information as to which of the moral faculties develops first and what the order is of those that follow. The child is only the future man as man himself is but a transient here, preparing for the spiritual life to come.

Throughout this period, and even preceding it we find biographies and autobiographies which are literary rather than truthful, fanciful rather than practical. They are often rated among the books on child study but to place them there is rather dangerous unless one realizes that they are a type in themselves. A good illustration of this tendency which shows by contrast with these other early studies how long-distanced the method is and how much less valuable and effective from the scientific standpoint is Goltz' "Buch der Kindheit (Selbstbiographie)" (64). This has been widely read by German parents and yet it is not scientific or even methodical in its treatment. This book appeared in 1847.

In 1851 Löbisch's work, "Die Entwicklungsgeschichte der Seele des Kindes" (106), appeared. This book seems to have had great influence if we may

judge by the number of times we find it quoted. Löbisch was a physician specializing on children's diseases and probably in many ways one of the earliest psychoclinicians.

The next significant publication was Sigismund's "Kind und Welt" (159). It appeared in 1856 and is one of the most delightful and suggestive books ever written on this subject. Studying his own child from birth through the periods of development of laughing, sitting, walking, and talking, Sigismund also compares its development with that of other children among his acquaintances. He gives the first suggestion of a genetic attitude and is not concerned as almost all before him have been, with educational applications. He touches upon the manifold topics of sensory and motor development while his observations regarding language and affective development are very creditable.

Heyfelder's (79) work, which is valuable chiefly because of his observations upon speech, appeared during the next two years. This is the first of the articles which tend more or less completely to select some one form of mental activity as their subject matter.

To Kussmaul (102) writing in 1859 we owe the first application of the extensive method to child study. Limiting himself to the study of the newly-born infant he makes the first generalized statements of mental development in young children that are based upon observations upon a number of cases.

From this time on the development of child study is much more rapid and the number of contributions becomes within a few years so voluminous that a complete bibliography of them, without any annotations, would far exceed this volume in size.

In 1869 the Berlin Pädagogische Verein issued a bul-

letin urging teachers to study the children entering school in order to ascertain their range of ideas and thus glimpse their individuality and range of experience. In spite of lack of enthusiasm and, in many instances, the inability to make competent reports, the results obtained were most illuminating and valuable. They showed the appalling lack of knowledge concerning things children were supposed to have grasped just because they were within range of observation. Individual differences were markedly shown, imitation was found to influence the group answers and yet the complexity and fascination of the findings reported by Bartholomai and Schwabe (9) were undoubtedly effective in stimulating further study of the child as he is before coming under school influences.

The work of Bartholomai and Schwabe was followed by that of Lange (103) in 1879 and the influence extended to America by the similar study made by G. Stanley Hall on Boston children in 1880 (70). This was the first systematic study of the mind of the child made in America. Similar work has since been done by Netschajeff (122) in Petrograd.

In 1873 Genzmer (54) published his verification of Kussmaul's study and this was followed in 1882 by Kroner's (101) work on the same subject. This physio-psychological study of the sensory development of the young child has become one of the most frequently pursued branches of child study. Several illustrations will suffice to show the height of this development. The recent work of Canestrini (26) is noteworthy. He not only uses an objective method but studies a large group, seventy, of children under both waking and sleeping conditions, with due regard for any change in the conditions of environment or in the child's physical

or emotional state.

Perhaps an even more careful study in an allied field, developed out of this, is that of Benedict and Talbot upon the "Physiology of the New-Born Infant" (11). They have studied the metabolism of one hundred newborn babies and have carried out their work in such careful detail that they can suggest certain procedures for the conservation of energy and supplemental feeding during the first week of life.

Taine's (169) brief note upon the acquisition of language in children and in the race is of interest for several reasons. It voices the genetic attitude and at the same time, since it comes to us from France, shows that Germany and Austria are not alone in progress in their new interest in the child. Moreover, this appeared in 1876 and a part of it was translated and published in the English journal *Mind* the next year. Darwin's "Biographical Sketch of an Infant" (34) appeared in the next number of the same magazine, its publication stimulated as he himself tells us by reading Taine's article. Written thirty-seven years before it was nevertheless, despite the delay, the first published product of English child study. This shows how little known and valued the earlier continental writers must have been among their British neighbors.

But bigger, more suggestive studies were soon to appear. In 1878 Perez published his book entitled "Le trois premières années de l'enfant" (132). The results of careful study are here combined with the logical, brilliant style of the French writer. Perez, while conforming to all rules for "scientific" writing of his time, yet gives such a fascinating study that the rapid demand for new editions of this book does not

seem remarkable.

He, first, seems to have felt the need of extending such observations over the whole of the pre-school period and so continues his study under the title "L'Enfant de trois à sept ans" (133). This is not quite as interesting as his first volume and has decidedly more of a pedagogical tendency but is worth noting because of its historical primacy in this field.

Meanwhile another contributor has appeared, one who usually, because of his greater value, has so overshadowed Perez that the priority of the latter and his consequent influence upon the time are overlooked. This is Preyer. In 1880 he published an article on Psychogenesis (140) which reveals him as an ardent evolutionist and consequently we can anticipate the treatment of his "Die Seele des Kindes" (141). He studies his own child from birth on and besides observation introduces a new element, experiments. These controlled experiences allow of variation at will. Especially noteworthy, although begun at a rather late age, are his experiments upon color to which he was probably stimulated by Allen's study of the color sense (1).

From this time on not only books but magazine articles upon child study and especially upon the child mind are so numerous that the detailed study of the development in any one country, or of any one line of investigation, would in itself necessitate extensive treatment. It seems more fitting here to indicate the high points in the chief branches of the many trends we find developing out of this period. Several of these have already been indicated.

Let us first survey the work in individual countries briefly.

In Germany Preyer continued to be the leading

authority for some time, interspersing his articles on the psychology of the child with work on the physiology of the embryo with especial consideration of the manifestations of life before birth (142). However we find him still writing in 1897 on the subject which was so well developed in his earlier work, that is on color discrimination (143).

The work of Ploss must not pass unmentioned. He has achieved one of those marvelous compilations which seem to need the German's plodding and indefatigable application. Ploss gives us the first study of the social customs built up around and influencing the child from the day he is born (136, 137).

The work of Vierordt (180, 181) must be mentioned, too. Although it is purely anatomical and physiological in both its subject matter and aims, it is the symbol of the great development of anthropological studies which give us the broader aspects of child study though they do not contribute directly to the psychology of the child.

Since then Ufer, Ament, Meumann and Stern have done the most significant work.

Ufer has not only written much himself but has also translated into German a considerable amount on this subject. Ament has been interested especially in child speech and in the history and development of child psychology in general. It would hardly be commensurate with the object of this sketch to attempt here, in a form which would necessarily be brief, any statement of the development which he has handled so well in his "Fortschritte der Kinderseelenkunde 1895-1903" (4).

Meumann, too, has contributed considerable to the upper end of this fore-school period but his aims are

more pedagogical.

Stern is undoubtedly the most important, most significant, as well as most voluminous German writer in this field—Gutberlet (69) calls him the greatest of all child psychologists of the present day.

The more recent writings, especially magazine contributions, indicate new workers who will probably supersede those just mentioned but whose work is not yet very well tested by time. Among these we may mention Dix (38), Schmidt (148), Buchner (21), Scupin (150), Dyroff (41) and especially Groos (66), while the careful, critical discussion of the psychology of development in Ruttmann's (146) survey of modern psychology is the most scientific and comprehensive summary so far available, altho it deals somewhat too exclusively with German contributions.

In Belgium the work has been confined to children of school age and the problems studied have been pedagogical in their formulation.

In France Perez continued writing up until his death but his discussions gradually became more philosophical and less based upon observation. Compayré's "L'Evolution intellectuelle et morale de l'enfant" (32) has supplanted Perez's work and is the most recent French publication which is general and comprehensive in its treatment.

Mere reference to the many valuable volumes of *L'Année Psychologique* will show the varied interests developed in this subject by Perez's countrymen. Binet and his collaborators, especially Henri and Simon, have accomplished much although nowhere do we find the same type of work as among the Germans. The French write brilliantly and convincingly but their technique is apt to be at fault. They seem to hit intuitively upon

right premises and conclusions, although their data may be unconvincing or scanty. The German work is more stolid, more convincing in its facts but less inspiring in application.

Among the lesser French writers of to-day we should perhaps mention Vaschide, Bouquet (18), Boutan (19) and Cramaussel (33).

The work on Child Study in Italy began with Luigi Ferri. Interested in the work of Darwin and Taine, he began studying a young child in regard to the development of sense perception, attention, speech, imitation, etc. His first results were published in 1879 (45) and were followed by others on the same child (46, 47). His work raises in its discussion some interesting philosophical questions.

Ottolenghi (125) studied skin sensitivity but largely on older children. Garbini seems to have done the most original and systematic of the Italian work. He studied the voice of the child (50), changes in its pitch, vibration frequency and duration of cries and he records the ages at which changes occur. He also did intensive work on development of the color sense (51), working on 557 children ranging from the new-born up to those 60 months of age. His work (52) on development of the olfactory sense on 10 new-born infants and 415 children between the ages of three and six is equally valuable. Garbini is undoubtedly an exception among those who have studied children. His work is thoroughly scientific and objective in method.

One other Italian writer needs special recognition and that is Paola Lombroso. Besides some original work on the instinct of conservation in children (109) she has written a historical and critical account of the work done by her fellow countrymen along the lines of

child study (108). The Italians have not so far produced any one study covering the whole period of childhood, Lombroso's book (107), which attempts this, being rather incomplete along some lines and quite limited in scope.

Russian literature is in general so little accessible that it is almost impossible to tell what has been accomplished in that country. Several names must be mentioned, though. Netschajeff has written quite a little in German magazines and has since 1904 had an institute for Paidology in Petrograd. Sikorskij is perhaps better known. The most recent of his writings is his "Die Seele des Kindes" (160) published in 1902. This has passed through three Russian editions but owing to lack of translation of the third edition it is impossible to state whether there are or are not new contributions in it. The many changes in and additions to the second edition lead one to feel that the author is still working towards, rather than resting on, an ideal study.

He does not, however, seem to be familiar with, at least he does not mention, the significance of the theories developed by his own fellow-countrymen, Pavlov and his followers.

In England there was rapid development in child study immediately following the publication of Darwin's sketch. Pollock's (139) work on language development of an infant appeared in the following year and was followed by the work of Champney (30) and Sully (166) on the same subject. With Sully this was the beginning of a long period of steady contribution to the subject and none of us needs to be reminded of his "Studies of Childhood" (167).

Warner has done quite a lot to advance the methods

of child study beginning with his publication of "The Children: How to Study Them" (183) in 1887. However, he aims chiefly at furthering educational methods, and studies especially the physical and physio-psychological aspects of the subject. One significant attempt of his should be mentioned. It is a brief report (182) of an apparatus which he designed and used to measure the spontaneous movements of the hands of children and adults. He found that in infants probably spontaneous movements "can be arrested by light and also by sound" (p. 162). He outlines a method for studying the character of the modification and reports that these modifications tend to occur regularly to the same stimuli, illustrating the statement with curves from his experiments.

Drummond (39, 40) is a more recent writer of a rather elementary type of book intended evidently for the inexperienced student. This type of contribution is rather well illustrated in the pages of *Child Study*, the official journal of the British Child Study Association, while the magazine, *The Child*, is devoted to the social, hygienic and educational welfare of the child. In England this practical tendency seems to have rather superseded the less material interest in child study itself.

There is, however, a small but valuable body of studies lying partly within the limits of child psychology. For instance McDougall's "Investigation of the Color Sense of Two Infants" (115) not only gives a higher development of method but is also a valuable contribution showing that color development not only comes earlier than Baldwin found but also that it is readily studied. Although different in technique the work of Myers (120) and Valentine (178) is similar.

On the whole English contributions to child study, in so far as it deals with the child of pre-school age, have been imitative rather than original and very scanty in number.

The situation in America is different and appears more so by contrast with England. As has been indicated the movement really began with Hall's work on the ideas of children upon school entrance which he made in Boston in 1880. Several minor magazine articles had appeared before this. Among these were Holden's (82) study of the vocabulary of children, appearing in 1877, and Humphreys (84) on the same topic, published in 1880.

But these articles excited little attention. Larger forces were needed to arouse the interest in child study. We see how Hall had started such a movement but before it attained its full force, which was not until students coming under his influence as President of Clark University began working in collaboration with him as well as independently, another influence was at work. The first work of Perez and then that of Preyer were translated and made more accessible to the public in general. Before the interest in these studies had time to wane Shinn's "Notes on the Development of a Child" (156) appeared and led to renewed interest in individual studies. Shinn's work although the first of this type of study in the United States is undoubtedly still its masterpiece, especially when her later studies are considered in connection with it (157, 158).

In the meantime many minor articles had appeared, but who to-day would recognize the names of Chaille (27), Calkins (25), Dewey (37), Allen (2) and Talbot (170) as authorities in this field?

From about 1890 progress seems very different.

Baldwin's first article on right- and left-handedness (5) appeared that year and was but the forerunner of his "Mental Development in the Child and the Race" (6), which is still a masterpiece of the followers of the genetic method. Earl Barnes began writing rather extensively about the same time and although educational in his aims he has done much to encourage child study.

The *Pedagogical Seminary* was established by Hall in 1891 and soon became almost synonymous as a title with child study contributions. Its pages are a history of the development of the study of the child by American writers. Among those interested in this subject we find Hall and his colleagues; Burnham, suggesting especially a scheme for classification of child study topics and since devoting himself more to the hygiene of the child, and Chamberlain, who led afield into the life of primitive peoples (28) but who has also given us a most careful study "in the light of the literature of evolution" (29). Later we have the work of Tanner (172) which pays more attention to the pedagogical significance of the various phases of child development.

Among those studying under the group just mentioned and coming under their influence many have made significant contributions to the study of the child in its physical, moral and educational aspects, while among those devoting themselves to child psychology we must mention Tracy, whose "Psychology of Childhood" (176) appeared in 1893, Kirkpatrick, whose work began in 1891 (94) and who has since given us two of our best introductory texts (95, 96), Gesell (55), interested in the young child from the educational aspect, and Patridge (127), emphasizing the study of the

individual.

Besides this group, originating from one center, we must mention a number of other authorities, Oppenheim (124), Jacoby (87), King (93), Major (113) and Thorndike (173). Another group follows the impulse given by Shinn's study. Here belong the "Study of a Child" by Hogan (81), Hall's "The First 500 Days of a Child's Life" (73) and Moore's "The Mental Development of a Child" (119).

The practical side of the work for the child is seen in Mangold's "Problems of Child Welfare" (114), while an interesting sketch of the child and the history of his social, political and humanitarian relations, by Payne (130), has just appeared.

The present tendency in the United States more than in any other country seems to be towards specialization of topic and method, with a great many theoretical as well as research contributions. The activity in this country resembles more nearly that of Germany than of any other country if we exclude the intense interest in "tests of intelligence" which deal usually with older children and wherein the French school leads.

We must not, however, think of child study as confined to the countries mentioned. Work is evidently going on in almost all civilized lands, but the difference in languages has made the results practically inaccessible. A report by Gilbertson (56) shows that the Danish Anthropological Survey has added quite a little to the data regarding the weight of new-born children and also concerning the so-called inferiority of first-born children. Grudzinska's (67) study of dolls, altho dealing largely with children of school age, gives us an indication of the work being done in Poland, while her report (68) of the Child Study Asso-

ciation of Warsaw indicates probably valuable work on language, ideas of space and first esthetic impressions.

Through Lippert (104) we find much is being done in Bohemia and Moravia and he reports that many of the colleges there give courses in child study. In Portugal we find Machado (112) has made a study of his child which resembles the biographical studies with which Germany and France both began. No doubt other studies have been made and the fact that the interest in child study is almost universal is seen by the fact that a First American Child's Congress was held in Buenos Aires in July, 1916.

CHAPTER II

METHODS AND RESULTS OF CHILD STUDY

WE may now justly ask what are the results which this child study movement has accomplished so far as the psychology of the pre-school child is concerned. The results may be grouped under several heads. First there is the development of methods themselves and secondly there are the facts that have been determined.

The subject matter of child study may include any and every thing that can be observed regarding the child's development, physically, mentally, functionally. The various fields of study may in general be classified as:

1. Anatomical studies or those dealing entirely with measurement of and description of the structure of the child's body, such as measurements of height, cephalic index, etc.

2. Physiological studies or those of the functioning of the child's organism. The many studies of the reflexes, sleep, blood supply, digestion and motor development belong here.

3. Physio-psychological or the studies of sensory development and discrimination, the development of motor control and the early language functioning belong here.

4. Purely psychological or studies of the development of memory, imagination, comprehension of lan-

guage and in general the expression of higher, more complex forms of thought.

5. Observations of behavior or the activity of the child as expressed in games, plays and spontaneous occupations, often with especial regard for emotional expression.

Of course two or more of these lines of observation may be pursued in a single study while some studies involve all five. All are interrelated and it is impossible to make a study, no matter how purely physiological or psychological, that does not influence and at the same time derive help from the other phases.

Any of these topics is a part of child psychology when it is dealt with from the standpoint of the significance of the factor as an indication of mental activity or as a correlative of mental activity. Anatomical studies may indicate growth and physiological age, also functional ability. In this aspect they, although they seem the most remote of any of these divisions from psychology, assume great probable significance if not as causative at least as correlative conditions of mental growth.

The method historically first, that is the diary, individual or biographical method, is the one usually thought of first. The conditions for its practice are the most universal—a baby, any baby, and an observer. Usually the observer is one of the parents or a relative. The method is slow and laborious, the results difficult of comparison and interpretation when obtained. It is impossible to record every conditioning factor in the environment that has led up to any recorded observation, hence the standardization of conditions for verifying the finding upon other children is impossible. The method is valuable as a basis for

genetic psychology and gives suggestions regarding the relative appearance of various mental phenomena. The work of Pestalozzi, Sigismund, Darwin, Perez, Preyer, Shinn, all illustrate this method but undoubtedly the Sterns represent it at its height. A brief summary of their views is consequently not only fitting but necessary.

Stern (162) designates the period of early childhood as that of play while the period of 7-14 is that of differentiation of work and play. Another difference lies in the fact that the young child is under home and kindergarten influences which vary greatly from the school surroundings of the older child. The nature of the young child's existence makes for observation of the individual over an extended period rather than for the observation of a great number of children. The pedagogue needs these studies of early childhood, needs to know the development of the material he works with. The work on the Stern children covers the first six years because Stern feels the need of filling in the big gap in our knowledge between what we know of the first three years, studied by so many, and school age which again is voluminously studied. He feels that a relative, especially the mother, with psychological training, is the best worker in this field and warns all others not to attempt working with the child until en rapport with it. He advises a study of only one group of developmental phenomena when one is first working in this field, but feels that the "picture will be one-sided and neglect much that is important" (162, p. 11). One should observe both spontaneous activities of the child and his reactions to stimuli purposely presented. The minor importance of experimentation he states very clearly.

"It must be mentioned that for us the experiment has far from the significance it has for the psychology of the school child and of the adult. For as our chief problem is the natural development of the child's mental life, we must ascribe observation of natural phenomena the chief place and avoid all which is destined to artificially influence and change development itself" (162, p. 12). Experiments are to be only casually introduced side lines to verify certain points, although they may be carried out on children who are not being continually observed, but even here care must be taken to not fatigue them.

Although in theory one can distinguish experiment upon a child from observation of him under natural conditions the differentiation in any one instance is very difficult. The experiment in general is a presentation of stimuli not present normally to all or most children of that age, but whether it is any more artificial for the son of a psychologist to play with colored balls, under observation, than it is for the daughter of a dressmaker to amuse herself with colored patches when observed only enough to keep her from harm is a metaphysical question. The fact that a child's every significant act is being recorded probably means that he is living in as artificial an environment when compared with other children as would be introduced in any purely experimental study.

In general we may say this method of prolonged individual study is the logical one for genetic psychology; the easiest from the standpoint of obtaining a subject; the slowest in obtaining results; the least satisfactory for evaluation and comparison, and apt to be less valuable because of its bulk and the possibility of its becoming too much permeated with the

personality of the compiler. The fact that almost none save German writers seems to have thought of using this method in the last decade shows that its usefulness was pathfinding, not terminal. Stern's work itself is largely valuable because of the experiments he adds.

A far better method, that of studying the genetic development of one type of mental activity, is the outgrowth of the biographical method. We see the beginnings of this in the emphasis Heyfelder placed upon language development in his study and in Preyer's especial attention to the development of color recognition. It is along these two lines of color and language study that most work of this type has been done. The mere definitizing a smaller field for study makes the results more definite, less voluminous and more readily studied. This method may be carried out on several children at the same time. In the work on language, the most highly developed branch of this type of study, we have no control of environment and hence there are great uncontrolled individual variations; such for instance as the number of words in the vocabulary of children of the same age studied by different observers. The Sterns (163) have made this type of study on their three children. The voluminous manner in which this one topic is handled shows how gigantic any treatise of all mental phenomena of development must be if the study be as detailed and accurate. Meumann's "Die Sprache des Kindes" (116) is also to be noted here as it is more theoretical and gives a basis for a working concept of the development of language in the child. In other words, he is more successful in generalization.

A great number of purely quantitative studies of the size of children's vocabularies have been made.

Some are more, some less complete. Where the progress in acquisition of words has been watched together with a record of the total vocabulary at each point we have a far more valuable quantitative and genetic study. Grant (65) in his study of his son's vocabulary and its growth gives a most concise, careful treatment, using this method. The comparison with other studies and the bibliography he presents are also good. But in spite of the number of such studies we have as yet no accepted or standard method of gathering the data, nor have we evolved any definite standards of the size, rate of growth, and range of the vocabulary of the so-called "average" child. We may surely ascribe this to the method which renders the task of gathering a great number of such vocabularies an impossibility for any one observer, while we lack sufficient trained workers to have put fifty or a hundred on this problem at the same time with the same method.

The study of the development of color discrimination is far more easily pursued under experimental conditions and it is to this subject that Baldwin early devoted his attention. In 1893 he published his suggestions for a new method of child study, calling the method by the term "dynamogenic." A full presentation of the method, its possibilities and the results obtained by the use of it were incorporated in his "Mental Development in the Child and the Race." Although this is chiefly an individual genetic study yet experiment plays a large part in furnishing data on the developmental processes discussed. Using the motor response or direct reflex in its simplicity he has ascertained the child's reaction to a varied series of situations. He recommends the use of the hand move-

ment as the most satisfactory and shows that through it the development of relative attractiveness of two stimuli; relative attractiveness (or better, stimulative ability) of two colors, two forms, or two brightnesses; the relative use of right and left hands; the development of imitative, voluntary and ancillary movements, as well as accuracy of estimation of distance may be studied. The method is used by the author to study color preference and the development of right- and left-handedness.

The interest of the author is, however, not centered in the experiments as such but he writes, "On the whole, therefore, I attach very little importance to the experiments apart from their illustrative value and their possibly stimulating effect upon others who may care to extend them. For these latter reasons, however, as much as for the positive inferences I have drawn from the above, I have felt that they ought not to be unrecorded. Their main purpose in the progress and plan of this book is seen in their witness to the regularity of operation of the principle of suggestion or dynamogenesis" (6, p. 57).

This method stimulated greatly the work on color discrimination and also that on the rise of right- or left-handedness. Results may be seen in the studies of McDougall and Valentine, already mentioned, as well as in those of Katz (92), but we must not forget that the work of Garbini on this subject is far more extensive and thorough, although he studied the evolution of the color sense by experimenting upon a number of children at different ages in preference to confining himself wholly to following its development in a smaller number of children.

The method of retrospection, or the use of autobiog-

raphies is far less satisfactory. The adult looks back upon his early childhood as an idealized experience. His attitudes, understanding, ideals are so different it would be impossible for him to give us a true picture of the child's attitude and ideals and thoughts. Such books as those of Una Hunt (85) and Pierre Loti (110) may give us a real insight into how adults regard their acts as children but they are not child nature naïve and simple. The work of Folsom (48) although dealing with a later period of child life is a suggestion which may lead to better things. The day-book kept by his mother and the records in his own writings give a basis of fact from which it is not so easy to stray. If we could have a biographical study of a child and then later his independent and spontaneous autobiography, if this might be done in only one instance, the relative value of the autobiography might be more readily estimated, but even then the probabilities are that its value would be for adult rather than child psychology, and would connect this method with another already in use, that of psycho-analysis.

Psycho-analysis is not, properly speaking, a child study method but again a method secondarily concerned with the things that have impressed the child and primarily a method of investigating the undue persistence in the adult of unfortunately conditioned associations and their emotional concomitants. By it we can trace the complex functionings of the adult back to infantile states but we cannot foretell in similar detail the development of such states in the child.

The methods mentioned so far are largely individual in their application, but there are several others which are distinctly applicable to a more or less extensive group. Of these the questionnaire or statistical method

has been the most widely used and with the most varied value in the results. Its characteristics fit it for a special type of study. The observations may reach into the tens of thousands, and come from a widely distributed group of observers. These are both advantages but precautions must be observed in its usage or there will be disadvantages also. Unless the questions are clear and specific the answers are apt to be less pertinent and hence less valuable. The questionnaire should not be too long or the demand upon the observer's good-will will outlast his attention. The evaluation of the results must allow for the use of an indirect method and not stress the importance of minor differences. Keeping these points well in mind the method has certain very specific values. Hall has used it with especial success to gather data regarding those forms of individual experience which can not easily be experimented upon in the laboratory. Especially significant is his work on fear (72). It is to the broad vision given by such studies that his volumes on adolescence owe much of their seer-like qualities. This method is also less apt to be unreliable if the data gathered are objective results such as children's drawings or if they have a numerical basis such as would be involved in a study of height, weight or counting ability.

Another method is the study of only one or two mental processes in a large number of children. If we contrast this with the biographical method we see that they stand at right angles to each other. The biographical method follows the child as he ascends in experience. The quantitative method takes any one point in his ascent, cross-sections it and studies its condition not only in the exceptional children but in all

children. One of the first pieces of work done in this way was that of Binet (13) on line comparison and number perception. This was done in 1890. Although his methods are very crude and the study was made only upon his two daughters then 32 and 52 months of age it is a beginning of his work which leads to two types of study, to a purely scientific study of the mental processes of young children, on one hand, and, on the other, to a standardization of processes possible at definite ages in order to estimate the relation of any one child's ability to that of the group for his age. These are the so-called "tests of intelligence." These two lines are really one and the same type of study. Any process that has been so thoroughly and voluminously studied that we know what ability in that line to expect of a child of any given age is a "standardized" process. Henceforth trying it out upon any given child is merely ascertaining his relation to the rest of the group, or in other words it is a "test." It will at once be seen, however, that this is true only under certain conditions, namely, if the original study has been made under controlled conditions which may be exactly reproduced when our individual child is tested. Such a method complies with the demands of experimental psychology and its use as a test fulfills the demands of applied, especially clinical, psychology.

Many studies have combined two or more of these methods such as biography and slight experimentation, biography with statistical and comparative confirmation, questionnaire and observation, developmental and group experiments. The results are of varied significance.

If we were to attempt the formulation of a textbook of the psychology of the first five or six years of life

on the same general plan as we would formulate a text on adult psychology we should find some phases so untouched by child study that all we could do would be to state our ignorance. In other phases the accumulated facts are so multitudinous that they would form whole books in themselves. This is not all due to the developmental and evolutionary character of child study material but also to the factors which largely influenced its development. Before evolution as a theory was advanced, there could be little need of studying the evolution of the child's mind more than just enough to determine when his "faculties" were sufficiently developed to make education possible and probable. Nor could experimental methods be used until experimental psychology had become a fact. Consequently the earliest work in child study was either the observation of his development for the purpose of founding on such evidence sound educational principles, as was the avowed purpose of Tiedemann and Froebel, or due to the purely scientific interest of a group of men not dependent upon psychological methods. This is the group with medical training, Löbisch, Sigismund, Kussmaul. They used the physiological method and although an exact knowledge of the time at which a child begins to show that he hears sounds or perceives light is less necessary for medical practice than for educating the child, the purely scientific interest in the child developed along these lines just because there was a method by which these phenomena could be studied while the avenues of approach to mental processes themselves seemed closed or lacking.

Consequently the studies on the development of the senses have been numerous and persist as a favorite subject for investigation in physiological psychology

to-day, although the method used has gradually passed from individual observation to a statistical, experimental procedure. This is easily seen if we compare the work of Kussmaul with the study by Peterson and Rainey (135), in which they studied 1060 children born in the same hospital and kept under the same conditions for the first few days of life. Various compilations of these findings have been made such as those found in the work of Tracy (176), Chamberlain (29), Cramaussel (33), Gaupp (53) and Tanner (171).

Besides sensory development many observations have been made upon the motor development of the child. Here again we must rely largely upon the individual studies. The age at which the child holds its head erect, sits, creeps, stands, walks, the appearance of tactual and distance prehension have all been minutely recorded by Sigismund, Preyer, Shinn and others. Some work on the standardization of the time at which these develop was done by Binet and Simon (16) in 1904.

The work of Dearborn (35) should be mentioned here. It is the study of a child's development but not made in the same manner as the other individual studies. The author keeps an objective attitude and, focusing especially upon the child's motor and sensory activities, records them day by day, keeping his comments differentiated from the actual observations. The arrangement and method give the book significant value as a modern scientific contribution and indicate many developmental features that should be investigated by the statistical method.

The data on memory is to be found chiefly in the pages of the biographical or individual studies which record the spontaneous utterance of recalled situations

and the recognition of persons and places known at an earlier period. Lately, however, we have the adoption of an experimental method and an already valuable literature on memory as studied through the Aussage method. We must note especially the work of Stern (161, 164), while the workers with mental tests depend perhaps too much upon the use of this method and the method of immediate recall because of development of norms of memory span for auditory presentation of digits, sentences, etc.

Correlated with this work on the Aussage we should mention that of Binet (14, 15) which gives the basis for Stern's work although it deals largely with older children.

Winch (186) made a widely extended study of observation and report upon English school children but includes infant schools, and, consequently, children from three to seven years of age. These were picked to represent bright, average and dull children, in all giving ten of each at each age. The three-year-old children could not correct their first Aussage concerning the picture but older ones could. The power of reporting grew more rapidly than the power of observation while the demand for production and reproduction of what was seen when the picture was originally shown did not improve the memory of it but rendered it more imperfect. The improvement in report is a steady one from the age of three up to seven but less regular above that.

The study of Ballard (7) on memory and forgetting touches only the upper end of this period. The youngest children he studied were five years old. He used simple verse and tested the amount they recalled and the improvement in learning. He found that younger

children improved more, and more rapidly, than older children but his results are open to some criticism as he used more difficult material and sometimes a different mode of presentation with older children.

Kammel (90) tries to make use of retrospection, under partially controlled conditions. He saw the subjects himself and explained his desire to them having them write their earliest remembrance. After three months he saw them again and asked for a report of any earlier memories. Out of 344 pupils from 12 to 20 years of age only four remembered any earlier event and only 16 changed minor details of the former record. He feels this method is better than the questionnaire method used by Miles (118), Henri (77) and others. The results are not very enlightening from the standpoint of child psychology. The earliest memory is most apt to come from the fourth year of life, although some come from as early a period as the second year. The fact remembered by the child is more apt to be aroused by some external situation than by his own person and is apt to be preserved in visual imagery and have a strong emotional background.

Another line of study, that of language development, is also one not needing very intricate methods but simply careful observation. Consequently the rise of this part of child psychology has also been independent of the development of modern methods. Through the study of language development not only is the acquisition of voluntary control of the vocal organs to be observed, but the psychologist reads into it and studies through it the beginnings of learning or formation of associations. The child first learns to associate the spoken word with the object or action. Then as he learns to say the word he applies it to the

object himself and gradually develops the ability to use groups of words. Whether his understanding of situations grows as his use of language grows is a matter of speculation. The objective evidence of language is in itself valuable for the genetic and comparative psychologist and also for the student of anthropology and sociology but it tells us little of the potentiality of the mind behind the expressions. Such great individual and group variations are found, probably due partly to environmental conditions, that the study of the language development is difficult enough without complicating it by attempting too many inferences regarding the conditions of its development.

Most of the language studies deal with its development through the first three years of life. The size of vocabulary and consequent increase in complexity of its usage make any complete study above that age rather difficult. There are some studies that deal with one phase or another of speech of the period from three to six but nothing complete enough to bridge the gap entirely. With the older children, especially those just entering school, other cross-sectioning methods are used. These are usually verbal association methods or the methods of definition and explanation of a list of words such as were used by Lange, Hall, Netschajeff and others in studying the contents of children's minds. The method, consequently, does not give direct information regarding language usage itself but rather regarding the comprehension of it and the studies have been largely pedagogical in their purpose. Pohlmann's (138) work is the best example of the application of this method, extending its usage on children of all ages up to fourteen. His work touches the fore-school period as it includes a study of six five- and six-year-

old children. This number, however, is small and he reports such a large percentage of admissions of ignorance of the words or refusals to answer that one doubts whether his method was applied with sufficient admixture of understanding of these younger children.

One other study, that of Engelsperger and Ziegler (43) should be mentioned here. Motivated by the studies on children entering school, they attempted a more scientific and exact study of not only the mental but physical development in a group of nearly 500 children just beginning school work. The psychological study, although confined to the development of the color sense, is very detailed. Color perception, matching and naming were studied and significant indications of the narrowness of range of discrimination were found. Interesting sex differences are also indicated by their tables, but so far as mental processes go we know but little more than we did before. The study is one of knowledge acquired, not of processes.

Aside from the work on memory and language and the study of the learning process thus involved very little attention has been paid to the process of acquisition itself. There is no literature available upon which to base a discussion of the learning process of the infant which would be similar to and supplement that of Meumann (117) on school children and adults.

The only other subject widely investigated which might be said to belong to child psychology is that of the activities of children, their games, arts and occupations. Few of these are subject to experimental control but we have an advantage in studying one form of activity which is objective in its results—this is drawing. A great deal has been done with the study of spontaneous productions of copies of objects and

of illustrations of stories. The studies of Ricci (144), Elmer Brown (20) and Barnes (8) are all valuable although they deal less with the children under school age than with those of school age. They do give us some idea of the things present in the child's concept, say of a man or a chair, but tell us little of how he develops that concept.

Even such a brief survey of the field convinces one that the number of studies made upon the mind of the pre-school child by the methods of experimental psychology are few. Why is this all psychology has had to offer here? There are at least three definite reasons.

The investigation of the child mind was first motivated by a desire to gather in this field facts that would help pedagogues. This aim naturally limited the majority of studies to children of school age, although the importance of a knowledge of the child of pre-school age has often been emphasized but even when so emphasized the attitude is not purely scientific and has been beautifully described by Burnham, who writes "The prime motive for such study has generally been the training of teachers in the observation of children. It has been done directly for the sake of the teachers; indirectly for the sake of the child, and incidentally for the sake of science" (23, p. 198).

Another factor is the difficulty of securing any considerable number of young children who will form a rather unselected group. This is evidenced by the lack of extended studies upon the periods between earliest infancy, where babies are available in maternity hospitals, and the kindergarten school age which again brings them together in a social group. The child of two, three or four lives in the home, not in a large group of his kind. Hence each must be sought alone.

The rise of interest in evolution has also tended to strengthen the natural conditions which make for the study of the individual.

But the most important reasons for neglect of scientific studies of mind in this period lie in experimental psychology itself. Experimental psychology is comparatively new, it is still in many fields working out more satisfactory details of procedure and apparatus for its adult subjects. The methods have evolved around introspection and are such as necessitate it as a part of the procedure. This at once makes it difficult for the psychologist studying the adult to see how the infant could be experimented upon since he surely can not introspect; although a species of report may be obtained from his older brothers or sisters. Ament, for instance, says that we can not experiment upon the young child with full satisfaction because the child is a growing person and the use of many experiments depends upon the development of the processes to be studied, while, he writes, "The possibility of experimenting with the child develops as the child develops" (3, p. 100). Only as the child approximates the mental equipment of the adult can we hope to study him in a similar manner.

Chrisman advised laboratory study of the child of three years or under in his dissertation in 1896. He notes the need of not over-fatiguing the child and insists that "no experiment dare be unpleasant or the least bit harmful" (31, p. 42), but when he comes to outline the work that is to be done in the laboratory he gives the directions for an elaborate anthropometric study and then confines his directions for mental observation to sensory discrimination, especially of color.

Peper (131), too, approves of the use of experi-

ments but in his attempt to give a list of experiments to be tried we find he mentions all those which are usually used upon older subjects and he gives no suggestions for modifications to be made for use upon younger children.

Groos (66) rather discourages any attempt at observation under controlled conditions because of the difficulty of simplifying our methods. He feels that if, however, we can apply an experimental method and at the same time keep the child entirely ignorant of our purpose it is the best plan to pursue.

Wundt represents fairly well the attitude experimental psychologists have held when he says "Animal and child psychology are relatively of less value when compared with the physiological disciplines of human and comparative history of development" (187, p. 6). But Stumpf (165) takes a more optimistic view and points out that although we can study the child only through his reactions to external and internal stimuli, yet this indirect method is probably balanced by the fact that the child shows his reactions with less dissembling and control than the adult.

Ufer, too, relies mainly upon prolonged observation of the individual although he does state that "Observation under experimental conditions also has its justification" (177, p. 70).

However, it is not until we come to the work of Katz that we find an ardent champion of the use of the scientific method in studying young children.

Katz points out in answer to Ament's statement of the impossibility of studying the child that "the possibility of experimenting with the child decreases with the development of the child" (92, p. 21). He points out that the mere fact that a young child cannot in-

trospect in itself constitutes a problem worth investigating while the impossibility of using the introspective method should not be thought to be synonymous, as it has been by Ament and many others, with the impossibility of studying the child by a method built up with especial regard for the child characteristics.

Katz feels that he has a method which is an infant psychology method and which will bring results which we can get in no other way, and he applies the experimental method in the study of the origin of color, form and size concepts and hence of abstraction. He used 29 children between the ages of two and a half and six and a half years of age and found no difficulty in keeping the children interested and happy.

If we wish to know how he first conceived the idea of using the experimental method with children we can find the answer in his earlier study with Révész (91). This was originally a study of the memory and learning of hens but in the interests of comparison they made a study of twelve children between the ages of one and a half and five years of age, using the same method, only substituting play counters of different colors for the different kinds of grain given the chickens. They also used a verbal Aufgabe but felt that the children, especially the younger ones, comprehended the situation more through the environmental conditions than through the actual verbal directions.

This leads us to ask whether the adoption of the methods of animal psychology is unique with Katz or whether others have made similar usage of it. The attempts so far are not numerous but they represent a new endeavor to reach a scientific method of studying the child and are consequently worth surveying.

Any usage of the child under methods of study

which approximate those of comparative psychology will not be handicapped by lack of knowledge of the mental processes of the child but will deal primarily with his behavior or his objective reaction to definite stimuli presented in a definite manner under controlled conditions. How much have the methods of behaviorism or objective psychology so far contributed to our knowledge of the child?

CHAPTER III

BEHAVIORISM AND CHILD PSYCHOLOGY

THE study of behavior has come to mean the study of any of the visible and external movements of the organism, whether man or the lower animals be the object of study. The explanation of the movements studied is made as far as possible in purely objective terms and subjective interpretations are carefully avoided. Animal psychology, since its development from the anecdotal stage, depends almost entirely upon this method but the study of behavior is not purely psychological. As Parmelee (126) points out the study of behavior involves the study of anatomical structure and physiological processes and is consequently fundamentally biological, while it is psychological when mental processes are involved and sociological when the behavior is influenced by association with other human beings.

The confusion of this distinction disappears, however, if we assume another point of view. Psychology is in reality only a part of the larger science of life in general which we call biology and hence assumes as its basis the presence of structure and physiological processes in the organism. Social reactions it does not consider as other than reactions to living stimuli and so essentially the same in process as the reactions to inanimate objects. This attitude is that adopted by present-day comparative psychologists and is well ex-

pressed by Watson (184) in his book on behavior.

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"Reflex, then, as a unit of analysis of instinct (as also of habit . . .) in the modified sense in which we use the term, embraces (1) the fairly definite and generally predictable but unlearned responses of lower and higher organisms to stimuli . . . We must be careful . . . not to overemphasize the concept of invariability and predictability, since depending upon the physiological state of the organisms we find, in extreme cases, the situation where a stimulus which at one time produces positive response may, under other conditions produce negative response . . . (2) We have in the case of both vetebrates and invertebrates many cases of highly unstable and indefinite response" (p. 110).

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of illustrations of stories. The studies of Ricci (144), Elmer Brown (20) and Barnes (8) are all valuable although they deal less with the children under school age than with those of school age. They do give us some idea of the things present in the child's concept, say of a man or a chair, but tell us little of how he develops that concept.

Even such a brief survey of the field convinces one that the number of studies made upon the mind of the pre-school child by the methods of experimental psychology are few. Why is this all psychology has had to offer here? There are at least three definite reasons.

The investigation of the child mind was first motivated by a desire to gather in this field facts that would help pedagogues. This aim naturally limited the majority of studies to children of school age, although the importance of a knowledge of the child of pre-school age has often been emphasized but even when so emphasized the attitude is not purely scientific and has been beautifully described by Burnham, who writes "The prime motive for such study has generally been the training of teachers in the observation of children. It has been done directly for the sake of the teachers; indirectly for the sake of the child, and incidentally for the sake of science" (23, p. 198).

Another factor is the difficulty of securing any considerable number of young children who will form a rather unselected group. This is evidenced by the lack of extended studies upon the periods between earliest infancy, where babies are available in maternity hospitals, and the kindergarten school age which again brings them together in a social group. The child of two, three or four lives in the home, not in a large group of his kind. Hence each must be sought alone.

The rise of interest in evolution has also tended to strengthen the natural conditions which make for the study of the individual.

But the most important reasons for neglect of scientific studies of mind in this period lie in experimental psychology itself. Experimental psychology is comparatively new, it is still in many fields working out more satisfactory details of procedure and apparatus for its adult subjects. The methods have evolved around introspection and are such as necessitate it as a part of the procedure. This at once makes it difficult for the psychologist studying the adult to see how the infant could be experimented upon since he surely can not introspect; although a species of report may be obtained from his older brothers or sisters. Ament, for instance, says that we can not experiment upon the young child with full satisfaction because the child is a growing person and the use of many experiments depends upon the development of the processes to be studied, while, he writes, "The possibility of experimenting with the child develops as the child develops" (3, p. 100). Only as the child approximates the mental equipment of the adult can we hope to study him in a similar manner.

Chrisman advised laboratory study of the child of three years or under in his dissertation in 1896. He notes the need of not over-fatiguing the child and insists that "no experiment dare be unpleasant or the least bit harmful" (31, p. 42), but when he comes to outline the work that is to be done in the laboratory he gives the directions for an elaborate anthropometric study and then confines his directions for mental observation to sensory discrimination, especially of color.

Peper (131), too, approves of the use of experi-

ments but in his attempt to give a list of experiments to be tried we find he mentions all those which are usually used upon older subjects and he gives no suggestions for modifications to be made for use upon younger children.

Groos (66) rather discourages any attempt at observation under controlled conditions because of the difficulty of simplifying our methods. He feels that if, however, we can apply an experimental method and at the same time keep the child entirely ignorant of our purpose it is the best plan to pursue.

Wundt represents fairly well the attitude experimental psychologists have held when he says "Animal and child psychology are relatively of less value when compared with the physiological disciplines of human and comparative history of development" (187, p. 6). But Stumpf (165) takes a more optimistic view and points out that although we can study the child only through his reactions to external and internal stimuli, yet this indirect method is probably balanced by the fact that the child shows his reactions with less dissembling and control than the adult.

Ufer, too, relies mainly upon prolonged observation of the individual although he does state that "Observation under experimental conditions also has its justification" (177, p. 70).

However, it is not until we come to the work of Katz that we find an ardent champion of the use of the scientific method in studying young children.

Katz points out in answer to Ament's statement of the impossibility of studying the child that "the possibility of experimenting with the child decreases with the development of the child" (92, p. 21). He points out that the mere fact that a young child cannot in-

trospect in itself constitutes a problem worth investigating while the impossibility of using the introspective method should not be thought to be synonymous, as it has been by Ament and many others, with the impossibility of studying the child by a method built up with especial regard for the child characteristics.

Katz feels that he has a method which is an infant psychology method and which will bring results which we can get in no other way, and he applies the experimental method in the study of the origin of color, form and size concepts and hence of abstraction. He used 29 children between the ages of two and a half and six and a half years of age and found no difficulty in keeping the children interested and happy.

If we wish to know how he first conceived the idea of using the experimental method with children we can find the answer in his earlier study with Révész (91). This was originally a study of the memory and learning of hens but in the interests of comparison they made a study of twelve children between the ages of one and a half and five years of age, using the same method, only substituting play counters of different colors for the different kinds of grain given the chickens. They also used a verbal Aufgabe but felt that the children, especially the younger ones, comprehended the situation more through the environmental conditions than through the actual verbal directions.

This leads us to ask whether the adoption of the methods of animal psychology is unique with Katz or whether others have made similar usage of it. The attempts so far are not numerous but they represent a new endeavor to reach a scientific method of studying the child and are consequently worth surveying.

Any usage of the child under methods of study

which approximate those of comparative psychology will not be handicapped by lack of knowledge of the mental processes of the child but will deal primarily with his behavior or his objective reaction to definite stimuli presented in a definite manner under controlled conditions. How much have the methods of behaviorism or objective psychology so far contributed to our knowledge of the child?

CHAPTER III

BEHAVIORISM AND CHILD PSYCHOLOGY

THE study of behavior has come to mean the study of any of the visible and external movements of the organism, whether man or the lower animals be the object of study. The explanation of the movements studied is made as far as possible in purely objective terms and subjective interpretations are carefully avoided. Animal psychology, since its development from the anecdotal stage, depends almost entirely upon this method but the study of behavior is not purely psychological. As Parmelee (126) points out the study of behavior involves the study of anatomical structure and physiological processes and is consequently fundamentally biological, while it is psychological when mental processes are involved and sociological when the behavior is influenced by association with other human beings.

The confusion of this distinction disappears, however, if we assume another point of view. Psychology is in reality only a part of the larger science of life in general which we call biology and hence assumes as its basis the presence of structure and physiological processes in the organism. Social reactions it does not consider as other than reactions to living stimuli and so essentially the same in process as the reactions to inanimate objects. This attitude is that adopted by present-day comparative psychologists and is well ex-

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ably with that of the child. Any comparison, however, seems rather arbitrary when we recall the conditions. The orang was striving for food, the thing he understood best, and the stimuli from the situation would be reinforced by habit and any hunger sensations. The child was given a verbal Aufgabe and the interest aroused in the banana was not the egocentric one of self-acquisition and self-satisfaction but an appeal to his sympathy for the orang. The situations are very different and the results naturally are not alike in the two instances.

The maze has been used with human subjects but no one has tried it on very young subjects. Hicks and Carr (80) used it upon children as young as eight years of age but below this age we find no records although the method is probably applicable if care be taken to increase the complexity of the situation gradually. There should be no difficulty about using the method without a direct verbal Aufgabe.

Hunter (86) used the method of delayed reactions upon raccoons, rats, dogs and children. There were five children in all, one two and a half years old, three six years of age and one eight years old. The method with the children differed radically from that used with the animals, the play attitude was suggested and the verbal Aufgabe given. The task was as follows: The child was behind a gate with the experimenter. On the opposite side of the room were three buttons which when pushed might make a noise or not as the experimenter desired. Above the buttons were electric lights. The child was told and taught through accommodating practice that if he pushed the proper button it would buzz and if he pushed this noisy button first he would get some candy. Then the light over some one button

was switched on and off, the child was detained a number of seconds, varying in the different trials, and then allowed to press the button. During the delay interval, which sometimes lasted twenty minutes or more, the child talked to or exchanged stories with the experimenter, drew pictures and in some cases of long delay was given candy.

The children had to acquire the association of the light and button for themselves and develop some method of retaining this associated factor during the delay period in any one trial. All save the youngest child learned the association in one trial. With her no errors were made after the seventeenth trial. In the delayed reactions two children who were six years of age experienced no difficulty until the interval had lengthened to more than four minutes. These two formulated their own "purpose to remember." The third six-year-old child was told the purpose of the experiments and had only one error in 15 trials, that on a 21-minute interval, although he succeeded on a 35-minute interval. This shows clearly the help given by the formulation. The two-and-a-half-year-old had great difficulty with even the 10 second period, failing 30% of the time although she did far better than that on periods as long as 50 seconds. These failures on the shorter intervals probably represent her failures to formulate a successful method of remembering.

The children are reported as being impatient and as fretting because of the delay although this unpleasantness was reduced and largely eliminated by the distractions during the delay period. The memory cue as to which button to press did not seem to depend at all upon a distinctive motor attitude but is due, Hunter thinks, to some intra-organic cue which does not persist,

due to the distractions during the delay period, but is revived at the moment of release. Whether this revived cue is sensory-perception or imaginal in content remains doubtful.

This is all that has been done with behavior methods in the study of young children. The results are rather discouraging in a way but are only what might be expected when they are the attempts of workers primarily interested in animal psychology. They make rather good usage of play and rewards but none seems to have been able to regard the child in a purely objective way as an animal with as distinctive characteristics as any other animal. Hamilton uses food as an incentive for his animals but substitutes toys with the infant. Katz substitutes the still more remotely affective stimulus of colored counters. Yerkes makes a similar error and all three introduce with children the additional factor of language. Hunter's work is better in his handling of the child but it, too, depends upon language development, while all the methods are cumbersome and unwieldy, and the results, so far, are vague, indefinite and complex.

But a more exact method of studying the less obvious behavior reactions of animals has been developed by another school. That is, by the group of Russian physiologists led by Pavlov. Their methods and results are less easily accessible and hence less well known in this country. What have they accomplished that may be utilized in the study of behavior?

In 1863 there appeared in the Russian tongue a work entitled "The Reflexes of the Cerebrum." The author, Setchenov, was far in advance of his time. He saw psychology as a part of physiology, saw it using the methods of the natural sciences and concentrating

upon the study of motor activity. Although his theories were somewhat crude, and not wholly in accord with the views of Helmholtz which were then generally known and accepted, they are most valuable. He formulated the theoretical basis of the Pavlov method of to-day.

"The psychical processes of man," he writes, "are, as is known, recognizable through outer phenomena, and usually the laity, as well as the naturalist and the psychologist, form an opinion of them by that means. Every one knows how great the world of these phenomena is. In this world are included the great variety of movements and sounds of which man is generally capable. And this whole mass of facts must be comprehended as far as possible and nothing left unconsidered. The problem at the first glance seems insoluble, but in reality it is not, and the reason it is not is as follows: The whole endless multiplicity of the outer manifestations of cerebral activity may be reduced to a single phenomenon, that of muscular activity. Thereby this question is considerably simplified. In reality it so happens that a milliard phenomena, which apparently have no connection with one another, may be traced back to the activity of a single group of muscles. We know that under the hand of the musician impassioned and mournful tones may be charmed from the lifeless instrument. The animating and creative hand of the musician and the sculptor execute in reality only a number of purely mechanical movements which, critically examined, may even be submitted to a mathematical analysis and be expressed by a formula. How would they be able under such conditions to impart the expression of passionate feeling to tones and pictures if it were not a purely mechanical act? There will surely

come a time when the outer manifestations of cerebral activity will be subjected to analysis just as the physicist analyses musical accord or the phenomena which are manifested by a freely falling body" (153, p. 3). In another place he writes: "Thought is the first two-thirds of a cerebral reflex" (154, p. 135).

The work of Setchenov was pioneer but it remained for Pavlov and his followers to work out through laborious experiments the first exact confirmation of his theories.

Under Ivan P. Pavlov, as Director, the work of all in the Physiological Department of the Institute of Experimental Medicine of St. Petersburg, was for many years concentrated on the study of the digestive glands. The first considerable announcement of the results appeared in the year 1897, in the book, published by Pavlov himself, entitled "The Work of the Digestive Glands." An English translation of this book was not made until 1902. In the preface of it Pavlov states that the book is "a joint work, the result of the principle, which actuates the whole laboratory. It owes its existence to the acuity of each individual, but in its totality to the guiding conception which has inspired us all" (129, p. xi). With the exception of several short reports this is the only statement of the work of the Russian school accessible in English. A brief résumé of their problems and results is therefore permissible.

The subject matter has been the physiology of the digestive glands. The reason for taking up this subject is, Pavlov states, a desire to replace the older, erroneous views presented in textbooks by a fuller and more correct representation of the work of these glands. The earlier work was based upon the secretion of the salivary and gastric glands, or the salivary and gastric

reflexes, as they were called. The method consisted in the study of the quantitative and qualitative modifications of the reflex which were conditioned by complex receptive and elaborative processes (psychic reactions) in the central nervous system. As Pavlov stated, "Natural science is under an obligation to determine only the precise connection which exists between the given natural phenomena and the responsive faculty of the living organism with respect to this phenomenon" (128, pp. 613-614).

The technique, involving, as it does, delicate and accurate surgical methods, is in itself a triumph and too well-known to need description. It will suffice to recall that from an artificial opening, drawing off from its natural function whatever secretion is being studied, there extends a small canula and tube by which the drops are either counted as they fall, or are measured by the scale on a graduate into which they fall, or else, as Nicolai finally developed the method, they drop upon the receiving arm of a Marey tambour and are recorded upon a revolving drum.

Along with the study of the differences in the secretory reflexes to various edible and non-edible substances the discovery was made that any phenomenon of the external world which is capable of impinging upon the organism through any one of the sense organs may become the excitant of the secretory reflexes ordinarily functioning in response to nutritive substances only. When the secretion is poured out by the glands as the result of the presentation of food the reflex is said to be a natural or unconditioned one. When, however, it functions in reaction to auditory stimulation by an arbitrary sound, or in response to a mechanical stimulation of the skin or to the presenta-

tion of a colored light or to any other stimulus not usually accompanying the feeding, then the reflex is said to be "artificial" or "conditioned." The stimulus which is the excitant of this conditioned reflex is called the "conditioned" stimulus.

The conditioned reflex is established as follows: At the same time that the dog (which is the animal that has been generally used in the Pavlov laboratory) is given a piece of meat or other food that stimulates the activity of the reflex, the stimulus which has been picked to become the associated excitant of the reflex is also presented. This simultaneous presentation of the conditioned stimulus and the natural stimulus (i e., the food) is repeated a number of times until at last the association has been so well established that the artificial, conditioned or unnatural stimulus, as it is variously called, will, when presented alone, excite the secretion.

These conditioned reflexes have several pecularities which have been verified by repeated experimentations. 1. They are unstable or inconstant. 2. They become ineffective upon repetition, inversely according to the time interval between the successive excitations of the reflex by use of the conditioned stimulus. 3. Obliteration of one reflex does not affect the functioning of others. 4. Spontaneous reappearance takes place only after one, two or more hours. 5. A conditioned reflex may be redeveloped by renewed association with the unconditioned reflex with which it was first associated.

The work of Pavlov himself seems to be purely physiological and medical in its interests. True, he speaks of, and experiments upon, the "psychic excitation" of the digestive secretions, but these so-called "psychic excitations" are merely all those stimuli which work otherwise than by direct temperature, chemical and me-

chanical excitations by the food entering the buccal cavity. The conclusion of his speech presented in the Huxley lectures on recent advances in science and their bearing on medicine and surgery shows this clearly. There he says, "The investigation of the conditioned reflex is of very great importance for the physiology of the higher parts of the central nervous system. Hitherto this department of physiology has throughout most of its extent availed itself of ideas not its own, ideas borrowed from psychology, but now there is a possibility of its being liberated from such evil influences. The conditioned reflexes lead us to the consideration of the position of animals in nature; this is a subject of immense extent and one that must be treated objectively" (128, p. 618). He ends with the prediction that these facts will throw light upon the "highest and most complicated portion of the animal mechanism" (128, p. 619).

This method has been very widely used on dogs but as its application involves the formation of a fistula for the study of almost any of the secretions it was thought to be inapplicable in the study of human beings. Modifications of method have arisen which are more promising. Kalischer (88, 89) trained dogs to eat when he presented certain tones, and to refrain from eating when all others were presented simply by associating the feeding with one tone only and found the dogs made no attempt to get the food he held in his hand unless the correct tone had been sounded. Similar methods might be used with young children.

To Bechterew (10) we owe, however, a more radical and valuable modification. The work of Pavlov though fundamental for the development of the theory of mental activity as reflex in its nature is too narrow in its

application to function as the basis of all psychology. Bechterew approaches the same subject from the psychological rather than the physiological viewpoint and hence applies his experimental data in that field. While Pavlov's work centers about the secretory reflexes, Bechterew studies the motor reflex. One method of studying it is as follows. The reflex movement of the withdrawal of the foot from metal electrodes through electrical stimulation is accompanied by the presentation of some other stimulus such as the flash of an electric light bulb, the ringing of a bell or the appearance of a colored form before the subject. After a certain number of trials the presentation of the arbitrarily chosen stimulus without the accompaniment of electrical stimulation will suffice to cause the raising of the foot. Voluntary movements are also used under various controlled conditions.

To Bechterew all activity is reflex activity or response to stimuli external to the organ or group of muscles reacting. He writes that he has observed in detail the development of reflexes in his five children throughout their earliest childhood but he gives us no statement of results other than a general theorizing regarding movements and language development as conditioned reflexes.

In this country Watson (185) has attempted an application of this method to human beings using Bechterew's method of electrical shock but substituting in his later work the movement of the fingers for foot and toe movements. The study is interesting to us because of its use upon one child, an eight-year-old boy. The experiment is reported as follows: "Whether the method can be used widely with children has not been determined. In the course of twenty minutes we ob-

tained the reflex several times upon an eight-year-old boy. When first punished he cried and showed some reluctancy towards having the experiment continue. One of the experimenters then sat in the room with him, and, under promise of a moving picture show after the experiment, the series was completed with smiling fortitude" (p. 102).

This simple report of what happened shows very significantly that the punishment method is apt to put the child into an unfavorable frame of mind and results obtained upon him will consequently not be comparable with those obtained from adults who understand the aim of scientific experiments. With younger children it is also highly improbable that the promise of some reward would carry them through the necessary number of trials and the mothers themselves would probably object.

One point is exemplified that can not be too emphatically stressed, no matter what method of studying the child is used. That is the need of getting en rapport with the child, of understanding him and knowing what will appeal to him and keep him interested in the experiment. The adult can be verbally directed towards the task that is set him in the experiment and by his acceptance of the direction is attitudinized towards the work expected of him and the value of the results for science will be sufficient as a goal idea. But the child wants immediate reward and also does not want to devote any of his time to things that are not innately attractive. He must be ruled by interest, not by forced attention.

Watson points out the application of this method in the study of sense perception, acuity and discrimination in all senses, memory and the so-called association

reaction work, also in determining sensory integrity in those who for some reason or other do not speak.

The application to the study of learning cannot come by this method unless it is modified. He gives five experiences with the shock and the other stimulus working together, then tries the conditioning stimulus alone. If it does not cause the reflex to function, he gives five more simultaneous stimulations before he tries it again. By this method he knows only that the conditioned reflex has established itself somewhere in the series of five, while a trial of the functioning ability after each punishment stimulus would give the exact point of development.

Another line of investigation using the Pavlov method of secretions was begun by Bickel and Bogen. Bickel had the opportunity to observe a 23-year-old girl with a fistula of the esophagus and stomach and found that, just as in the Pavlov dogs, "The different stimuli, which touch the taste or smell organ, suffice to cause a secretion or increase a secretion already in flow" (12, p, 591). He did not, it seems, attempt any serious experiments in an effort to develop conditioned reflexes.

Bogen (17) was familiar with some of the earlier reports of Bickel and consequently when a similar case presented itself in the Children's Clinic at the University of Heidelberg he studied it not only for the reflex secretion to natural stimuli but for the artificial or conditioned reflexes.

The case was a boy three and a half years old who had drunk some lye and gradually developed complete stegnosis of the esophagus so that finally a stomach fistula was made and used for feeding while curative measures were attempted. The experiments were car-

ried out while the child was in the hospital.

The child was laid on his stomach on two outstretched towels which did not meet but allowed the canula to pass between. The child could not see what was going on in the room. Then he was fed milk or meat and the gastric justice would begin flowing although the food was regurgitated as it could not reach the stomach. "After about 6 such experiments had been made it also happened that the sight of the meat as well as of the milk called forth a psychic secretion of gastric juice" (p. 736). Then the associative experiment was used. "The child was fed a long time—in all over 40 times—with meat, while simultaneously a certain tone was blown upon a small trumpet" . . . Other combinations were also used, the showing of food and blowing the trumpet, etc. and "all these experiments gave positive results" (p. 737). Finally in ten trials of the blowing of the trumpet seven were followed by secretion and only three were negative. Anger and pain delayed the secretion, the period of latency for meat was four and three-quarters minutes, for milk, nine minutes, and the secretion decreased as the intensity of stimuli decreased.

So far we see little indication of a method which can be easily applied to the normal child of any age independent of his ability to speak or understand the spoken word, or readily adapted for the study of many processes, one that does not force the child into an unpleasant situation but that invites his coöperation through arousing his interest and which nevertheless is thoroughly in accord with scientific theories and is able to evoke through its use data scientifically valuable.

From the work of Pavlov, Bechterew, and Kostyleff

(97) who has given the theoretical application of the idea of the conditioned reflex its highest development; from the work of Verworn (179), Sherrington (155) and Loeb (105), and from the work of the behavior psychologists already mentioned we have, however, evolved a theory of the child which we can use to develop the working rules for the scientific study of him.

The child, becoming at birth independent in his existence, is an irritable organism with a potentiality for many diverse forms of activity. This irritability is at first not highly specific but practically any stimulus calls forth a diffuse reaction. Within the organism, however, certain reactions have already been determined in a very specific manner by the completed development of the neural paths subserving those reactions. These are the first instincts. Gradually specific neural paths or neural habits of reaction develop for many more and far more varied stimuli. These reactions are instinctive if they are independent of the experience of the individual previous to their functioning, habits if they are dependent upon his earlier experience.

Any stimulus in the environment of the individual may become the excitant of a reaction which may become habitual. The type of reaction which any one stimulus may excite is not specific but is of an excessively varied potentiality. Any reaction of the child at any time is dependent not only upon his environment at that time but also upon the sum total of his earlier experiences and likewise not independent of his effectively inherited predispositions, that is the specific tendencies or incapacities of his organism.

The complexity of the conditions is increased by the fact that the child is a growing organism. Habits are formed, specific reactions developed, not only because

of the plastic receptivity of his organism but all activity and functioning is favored by the very nature of the child himself. His fundamental necessity is growth. The great strides which the most casual observer notes in the young child are the result of simultaneity of growth in structure and growth in function which we call adaptation or learning.

From this we may formulate the theorems basal for a scientific method of studying child behavior.

1. The child is a responsive organism.

2. Response may be structural and so involves growth.

3. The study of structural responses, as a type of adaptation and learning, is the necessary concomitant of the study of neural response. The two are not antagonistic. The presence of one does not, as has often been claimed, impede the study of the other. They are necessary correlates and their correlated presence distinguishes the study of the child from the study of the adult which is largely one of interest in function.

4. All neural response is of the reflex type, that is, direct response to a stimulus or group of stimuli.

5. The neural response is never arbitrary but always motivated by a definite stimulus although for any one stimulus the response may vary greatly, and, vice versa, any one response may at different times be caused by widely different stimuli.

6. The child responds to all in his environment for which sense specificity has developed but primarily to the most effectively sensed stimuli.

7. Any stimulus quality in the external world may be brought into causal or inhibitory relation with any reaction or group of reactions of the functioning organism and probably none is without structural effect.

The next question is as to the methods by which this theory may be applied to the study of the young child. Baldwin in his dynamogenic method comes very near giving one solution, while Warner's work on hand movements approaches a similar and more exact solution of the same thing. But neither of these men had the apperceptive background of method and knowledge of the conditioned reflex which in Krasnogorski led inevitably to its application to the study of children. His work far exceeds in suggestiveness anything preceding it, although it should be mentioned that the work of Bickel and Bogen seems to have been an incentive to him. His method and its application are, however, his own.

CHAPTER IV

THE EXPERIMENTS OF KRASNOGORSKI

THE first definite application of the Pavlov method to normal children was made by Krasnogorski.

In 1907, in the *Russki Wratsch* (98), he presented the results of a study of conditioned reflexes as developed in sucklings. Using one fourteen-month-old baby, he studied the cortical activity as indicated by salivary secretion. He had the child in a room by itself, lying on a table, no noise or other changing factor being allowed to disturb the conditions, and even the movements of the experimenter being kept as constant as possible.

After a few trials in which the child was excited by the sight of food (milk in a glass) held at a distance there was an increasing frequency of swallowing movements and also a motor reaction of mouthing and sucking. Krasnogorski felt that this presented such a specific picture that it might well be used to study the conditioned reflexes in normal cases where there was no salivary fistula by means of which the salivary secretion could be studied.

In children even the most unimportant salivary secretion, of even 0.5 of a square centimeter, induces the act of swallowing and by the number of swallowings in a given time one can judge the force of secretion. In this instance the child studied had been fed and had then enjoyed a short rest before being brought under the experimental conditions.

A glass with milk in it was shown with the following results:

RECORD OF NUMBER OF SWALLOWS IN THREE MINUTES

	First Observation	*Second Observation*	*Third Observation*
Before stimulation	1	3	3
	3	3	2
	0	2	3
	2	1	3
Excited by food	9	16	10

When the auditory stimulus of a ringing bell was used the following results were obtained in a 3- to 5-minute interval after the ringing of the bell, "after a certain number of trials" (he does not state the number).

RECORD OF NUMBER OF SWALLOWS IN THREE TO FIVE MINUTES

	First Observation	*Second Observation*	*Third Observation*
	3	3	5
Before excitation	4		
	2	3	4
	2		
	0	2	4
During stimulation	6	8	8

From these observations Krasnogorski concludes:

The amount of secretion in response to the auditory stimulation is in quantitative relation to the amount secreted during the foreperiod.

If the unconditioned stimulus used is one producing a relatively great amount of saliva then the amount obtained from excitation by a conditioned stimulus built up upon this basis is correspondingly great as measured by the increasing frequency of swallowing.

If the child cries when he sees food and does not get it this interferes with the conditioned reflex, having an unfavorable influence upon it, and the experiment can be resumed with favorable results, only when the child again becomes quiet.

Later Krasnogorski modified and perfected his procedure considerably. A first full report of the findings with these improved methods was made by him in 1908 at the meeting of the Society of Children's Physicians in Petrograd.

He had been studying the secretion of saliva by counting the number of swallowing movements which, being dependent upon the secretion itself, might naturally be expected to vary with it in intensity and duration. But there intervened a period between the first stimulation by a food quality and this movement, a period in which saliva was being secreted but had not yet accumulated in sufficient quantity to stimulate the swallowing reflexes. Other movements were present and noticeable, however.

These were movements of mouth opening as preparation for food reception. Accordingly he changed his observation to a record of the combined movements of mouth opening and swallowing. These were recorded by the simple arrangement of the receptive plate of a Marey tambour placed over the thyroid cartilage or the hyoid bone, connected by a rubber tubing with a recording arm which traced each movement upon a kymograph.

The child, with recorder in position, was "placed in absolute quietness upon a table in an isolated room. The eyes of the child were bandaged to avoid visual stimuli, if the conditioned reflexes were developed from various other receptive sensory surfaces" (99, p. 10). Taking first a control record of a quiescent state he next stimulated the child by feeding chocolate or honey. In the curves recording this feeding the initial deviation marking mouth opening may be easily differentiated from the swallowing records following. Such a "natural" or unconditioned reflex is too complex to allow of satisfactory analysis, consequently the effort was made to establish "conditioned" or "artificially developed" reflexes.

Three children were used, one three years old and two six years old. First the conditioned reflex was established in the 3-year-old by the ringing of an electric bell for one minute, every third minute with the accompanying feeding of one-half teaspoonful of honey fifteen seconds after the ringing started. This was repeated until the ringing of the bell was sufficient to cause the feeding movements. The association lasted and functioned two weeks after its last developmental functioning. The same conditioned reflex was developed in one 6-year-old and the "reaction after 24 hours was weaker than a second reaction 25 minutes later" (99, p. 13).

In both 6-year-olds the reflex was developed to the sounding of a tone on the reed pipe. Chocolate was given 10 minutes after the beginning of the sound. Although the note gave the reflex action yet similar reaction occurred to other tones. Seemingly, tonal discrimination is not as well developed in the child at six as Seleni and others find it to be in the dog. Nor

did re-enforcement of only the response to the correct tone by chocolate feeding seem propædeutic of increased tonal discrimination.

Krasnogorski also used cutaneous stimulation, scratching a place on the skin lightly with a camel's hair brush for one minute and, after 15 seconds of the mechanical irritation, associatively feeding chocolate. This stimulus soon became an adequate excitant of the feeding reflexes. But in the 6-year-olds the stimulus was not spatially specific, the same response in the reflex followed scratching upon the foot as had been developed to scratching of the arm above the elbow. Spatial specificity of excitation of reflex was secured in the 6-year-olds, however, by alternating stimulation of the particular spot first used and feeding, with stimulation of other portions of the body without feeding. In the 3-year-old child the stimulation had a specific and local value from the first.

The question of the decadence or unlearning of the conditioned reflex next occupied Krasnogorski's attention. A 6-year-old was put in a quiet position and the child stimulated by sight of food held before him without his being fed. This was continued 30 seconds. After 5 minutes a second stimulation was similarly conducted, a third, and then on the fourth no reaction occurred. Then the child was given some honey and the reaction immediately reappeared.

With the other 6-year-old, where a conditioned reflex had been developed to the ringing of an electric bell every 10 minutes for 30 seconds, the disappearance took place gradually being completed in five trials. When the pauses between stimulations in this same child were only 5 minutes the inhibition occurred after three trials. The reflex was renewed by one associative func-

tioning of conditioned stimulus and feeding.

The breaking down of the reflex as conditioned by mechanical stimulation occurs with a 6-year-old child after 3 trials, being absent on the fourth and fifth trials.

A second series of experiments was carried out in which the working of the feeding, or natural stimulus, and the artificial stimulus instead of being simultaneous were successive. Mechanical stimulation was used, for example, and after its application for 30 seconds there was a pause of 10 seconds and then the child was fed in the 11th second. After 16 such associated functionings the scratchings acted as a conditioning factor in the 17th trial. This reaction occurred during the early part of the scratching period, but during subsequent repetitions it appeared more and more slowly until it was present only after the cessation of the scratching. The child was lively and very exceptional in endowment and attempts to break down the conditioned reflex were very slow in taking effect. It took eleven functionings without feeding before, on the twelfth stimulation, we find a complete dissolution of the reaction. In disappearing the reflex maintained the temporal position developed earlier of functioning *after* the period of stimulation.

Krasnogorski did not in this study ascertain the value of reflexes during sleep or relative values at different ages although he states that in decadence the reflexes are "different in different children" (99, p. 24).

In 1913 Krasnogorski presented a second report of his work at the International Medical Congress held in London. Evidently, in spite of the brevity of his report, the results he has achieved are conclusive to him for he says, "Each phenomenon of the external world,

which is received by the peripheral systems of the child, can be brought into a temporary association with a motor, that is a secretory, act. All possible stimuli of sight, hearing and skin can be metamorphosed into specific excitants and call forth a definite motor act if their effect as stimuli is temporally associated with the motor act several times" (100, p. 376).

Without giving detailed results (or in fact any specific data) he states that in normal children 2-10 trials is sufficient to establish an associative functioning; that the conditioned reflexes so established are of "high stability" and "easy inhibition," but that there is a wide range of innate individual variation and he also intimates, but does not actually state, a difference due to chronological age, for he writes: "In normal infants the cortical innervations are so little developed in the first two or three months of life that the conditioned associations usually can not be developed. But as early as the second half of the first year of life the development of temporary associations from all receptive surfaces (eyes, ears, nose, skin) is indeed possible, but occurs more slowly than is the case in later life. Only in the course of the second year of life does the mechanism of the conditioned reflexes reach its full development and functional perfection" (100, p. 376-377).

Moreover Krasnogorski asserts that all sorts of pathological states cause changes in the activity of the conditioned reflexes; fever periods reduce activity, while "In many cases of idiocy, in neuro-psychopathic children, in cases of organic lesions of the cortex the development of the temporary association is either entirely impossible or else very much more difficult" (100, p. 377).

The dissolution of these conditioned reflexes is also

very interesting and exceedingly valuable Krasnogorski believes. In neuropathic children this process is unusually difficult. He gives as an example the case of a five-year-old boy where the conditioned reflex in response to cutaneous stimulation developed "proportionately easily" and where thirty-one repetitions of the conditioning stimulus without accompanying feeding were necessary before the dissociation was effected.

The memory-conditioned reflexes are also more fully treated in this report. Unlike the memory conditioned reflex of the dog which lacks specificity and disappears easily "high specificity and extraordinary precision are usual characteristics of this group of the reflexes in human beings . . . the memory reflex is formed just as easily as the usual contemporary associations. It is sufficient to cause the memorial effect of some sort of a stimulus to occur together with the opening of the mouth only 20-30 times in order for these memory traces to become adequate excitants" (100, p. 379). As this memory reflex develops at a later age, shows lessened specificity in many neuropathic children, and is more difficult to develop in imbeciles and morons its clinical significance is great.

The next mechanism which Krasnogorski has investigated is that of the analysers. Analysers, according to the Pavlov and Krasnogorski terminology are "those neural apparatus which analyze and resolve the stimuli of the external world into the minutest parts in order to build up from these fractional bits new combinations which represent the regular projection of these or those external phenomena" (100, p. 380). Dogs have the auditory analyser perfected to the discrimination of differences of an eighth of a tone while with children it is very poorly developed, but with them the visual an-

alyser is much more highly developed than with the dog. Red and white lights are discriminated by 5- to 6-months-old normal infants, odors are differentiated without great difficulty at the age of 7 to 8 months while the skin analyser is well developed towards the end of the first year. The movement analyser or motor zone is very poorly developed during the first year, but reaches its full development in the course of the second year of life.

Analysing ability is greatly decreased in idiot and imbecile children, may be disturbed temporarily or permanently in neuropathic conditions, appearing less readily and fluctuating more easily. A most marked example of disturbance of the motor analyser is to be found in the hysterical paralyses.

"The clinical investigation of the ability of the analysers to differentiate as made by means of the conditioned motor reflexes appears to be the sole objective clinical method which permits the determination of the analyser activity of a definite portion of the cortex as well as the discrimination of pathological processes.

Each neural process consists, as is known, of the phenomena of stimulation and of innervation—they are in a manner two halves of one and the same activity of the nervous system. The normal course of these nerve processes depends upon the balancing, upon the equal force, of these two forms of energy. Consequently it is comprehensible what unusual significance appertains to the mechanism which lies at the bottom of cortical innervations" (100, p. 384-385).

The method of conditioned reflexes, because of the delicate fluctuations in the reflex due to changes in the organism gives us an excellent measure of the interplay of reactions, of inhibition, and discharge in any one cor-

tical center. At first the stimulus of any one sense excites the whole cortical area of that sense and all such stimulation is followed by the associated reflexes of feeding. But as only one part of that sense organ is stimulated in association with the natural conditioning of feeding gradually the rest of that group of cortical analysers develops an inhibitory condition and only the selected and specific stimulation arouses the reflex. This specificity of reaction is rapidly lost and is very unstable. "One must assume," writes Krasnogorski, "that *the whole cortex is filled with such conditioned centers, which quickly arise, quickly disappear, quickly develop in size, soon grow smaller and are always bound up with this or that system of the cortex according to the conditions of their arousal*" (100, p. 388).

The formation of this specific conditioned reflex in animals passes through three stages. First the attempted inhibition brings accompanying inhibition of the desired reaction also, then "stimulation of the inactive part induces a reflex but its disappearance is not followed by the disappearance of the reflex from the active part; in the third phase . . . the phase of absolute differentiation, the inactive stimulation excites neither secretory nor motor reaction any longer, although the stimulation of the active part shows its maximum result" (100, p. 388). In children, however, Krasnogorski finds the first two phases little differentiated and absolute differentiation rapidly develops except in very young infants and in neuropaths where the second phase is almost always clearly lengthened. In idiocy, imbecility and myxoedema "the formation of conditioned centers takes place with unusual difficulty" (100, p. 388) while in excitable neuropathy "the stability of the conditioned centers appears greatly reduced"

(100, p. 388). The "inner inhibition" is more inert in young children and only reaches its functional maturity in children between three and four years of age.

Another mechanism discovered by Pavlov's laboratory and used by Krasnogorski is that designated as a "conditioned inhibition." By associating any stimulus with the working of a previously established conditioned reflex, without accompanying feeding, the new stimulus becomes in a short time the adequate stimulus for inhibition of the action of the conditioned reflex which it accompanied but this conditioned reflex functions as usual when the inhibiting stimulus is not applied.

This inhibition Krasnogorski finds develops in normal children in from five to ten trials. It then has a strong inhibitory effect, breaks down easily and disappears slowly if left to time. In gross pathological cases, however, the inhibition develops more slowly, is less effective and disappears very quickly. In neuropathic children the inhibition behaves differently according to the type. The excitable neuropath develops the inhibition easily, but it breaks down easily and disappears rapidly under lapse of time. In the phlegmatic child it develops slowly and only in response to relatively strong stimuli and it may, although not always, last a considerable period.

The most complex mechanism evolved by Krasnogorski is that of "loading and discharge." A conditioned reflex is developed in response to a stimulus through any sense organ and any chance development of a conditioned reflex in response to stimulation of some other sense is broken down by the repetition of the stimulation without feeding. Then the conditioning stimulus is given and no feeding but a presentation of

the non-conditioning stimulus follows and the feeding follows it. Then the non-conditioning stimulus is given again without accompanying feeding. Thus the reflex develops in sequential order as response to the first stimulus, formerly indifferent, after the conditioning stimulus. Krasnogorski used ringing a bell one-half minute, then 3 minutes later stimulating the skin. "In this case," he writes, "the skin stimulation appears, so to speak, as the trigger of the charge weapon. We load the neural mechanism with the stimulation by the ringing of the bell, this lading remains in the cortex and can be discharged at a favorable moment, if we set in motion our depressing mechanism—the skin stimulation" (100, p. 391-392).

In view of the clinical significance which Krasnogorski claims for this mechanism it is worth while to give here his physiological explanation of it. "The elements of the hearing analyser were brought into a co-temporal association with the motor cells. Then we changed the conditions of our experiments in that we did not let the unconditioned stimulus (the giving of chocolate) occur together with the ringing of the bell, but with a stimulation of the skin following a little afterwards. Consequently an inner inhibition was developed in the hearing analyser which restrained the energy of the bell stimulation in a latent condition, did not permit it to pass over into the motor elements. In contrast the cortical elements of the skin analyser are in no co-temporal association with the motor elements —as we have seen, the skin stimulation as such arouses no motor act. The skin analyser is exclusively in a co-temporal contact with the inhibiting mechanism of the hearing analyser. Therefore it is clear that as soon as the skin stimulation is received and the elements of the

skin analyser are set in motion, the wave of the stimulation (the positive energy) must tend over the paths developed towards the inhibiting mechanism of the hearing analyser. The inhibiting energy (negative energy) which is concentrated in the hearing analyser is thereby neutralized and consequently the inhibiting mechanism is weakened and the charge of positive energy of the hearing analyser is freed from the inhibition. This energy of the hearing analyser will be conducted, by the temporary associations by which the hearing analyser is connected with the motor element, to the latter and will call forth, as we have seen, a motor reaction" (100, p. 392).

This mechanism was developed in a five-year-old child. A cutaneous conditional reflex was developed and then a ringing of a bell introduced with skin stimulation 2 minutes later. No reaction appears but to the next skin stimulation ten minutes later a reaction is evident.

This mechanism does not function before the end of the second year of life and only reaches its full development in the third year. In excitable neuropaths this mechanism develops rapidly but also disappears easily. These are the children who learn rapidly and forget just as readily. In the phlegmatic neuropath the mechanism forms slowly and then in some imbeciles is very unstable while in others it is persistent. Some children who learn very readily also keep the acquired reaction very persistently.

These six mechanisms, co-temporally conditioned reflexes, their inhibition, memorially-conditioned reflexes, specific memory reflexes, ability of analysers, lading and discharge, give us an objective measurement of the child's neural assets which must replace the so-called

intelligence testing. They will also give us a definite means of clinically separating the pathological cases in early childhood.

Krasnogorski asserts also that (in epileptics) their functioning will indicate the onset of a convulsion and pass through four stages of re-development of their power of functioning after a convulsion. In myxœdema the reflexes develop slowly and disappear rapidly. Thyroid treatment does not change the manner of functioning of these reflexes.

There is a diminution of activity of these mechanisms in the beginning of tubercular meningitis and this decrease is constant and progressive.

By means of the study possible at an early age by use of these mechanisms defects discovered may be correctively and educationally dealt with long before they are usually noticed and the possible value and significance of this training may be enormous.

CRITICISM OF KRASNOGORSKI'S STUDIES

Promising and valuable as Krasnogorski's work may appear to even the casual reader, no one would deny that it is suggestive rather than decisive, initial rather than final and inspirational rather than scientific so far as the data published are concerned and there are several points open to criticism.

In his statement regarding technique Krasnogorski shows he has eliminated all disturbing stimuli from the external world by ensuring a quiet room and a quiet subject in a position of relaxation. One important factor he has entirely neglected despite these precautions. That is the child himself. No mention is made as to how he was induced to assume the position, as to

whether he was familiar with the experimenter, what his emotional reaction to the situation was, what his attitude mentally was, that is, was he bribed or threatened or coaxed. In fact that whole externally-undetectable group of factors is not even hinted at although more detailed statements as to how the child was handled might throw some light upon the results obtained.

No mention is made by Krasnogorski of the criteria used to determine the mental development of his subjects. Idiots, imbeciles, cretins, hysteria cases, neuropsychopaths, epileptics, have all evidently been studied as well as normal children, but of how many and of what ages, degree of pathological characterization, and hereditary predisposition we can unfortunately find no statement.

In his work to determine pitch discrimination Krasnogorski used a reflex developed upon a feeding delayed ten minutes after the "beginning of the sound." How long the sound was kept up we do not know, but the conditions seem rather unfair either way. If the sound were continued half the time subjective changes such as those due to fatigue, anticipation and fluctuation of attention would make final discrimination of the sound difficult when compared with another at least five or ten minutes later. If the bell rang or tone sounded only a few seconds the difficulty would be to tell to what extent the child remembered and how far he forgot the stimulus in the eight- or nine-minute interval. Problems on tonal discrimination undertaken with adults report great difficulty in discrimination when tones are immediately successive.

Although he states that the conditioned reflex is very difficult to develop in children under one year of age, yet Krasnogorski has used it to determine color discrimina-

tion at the age of six or seven months, showing that that mechanism *can* be developed at such an age.

The time intervals in the various experiments differ without any seeming rationality. In one series the stimulation was of one minute's duration and with two minutes between successive stimulations, and feeding was fifteen seconds after initiation of the stimulation. In testing dissolution of the reflex thirty seconds of stimulation at five minute intervals was used. In another case the reflex was developed by thirty seconds of stimulation applied every ten minutes, while dissolution was attempted with both five- and ten-minute periods.

Since in this latter case the results obtained on the same child varied with the time interval we must consider this interval an important factor. In order that results may be at all valuable for comparison it must be kept as nearly constant as possible. If one develops a conditioned reflex with a minute's stimulation once every three minutes and breaks it down with thirty seconds of stimulation every five minutes there are two variables in the time factor alone and the value of the variation between development and dissolution cannot be calculated except through results obtained by numerous series of experiments. Undoubtedly a minute's stimulation is different from a half minute's application of the same stimulus, but whether the additional thirty seconds strengthens the effect, whether it merely balances fatigue or whether it lessens the effect through adaptation can not be settled a priori. The length of interval, too, seems to affect results but whether three or five or ten minutes is most conducive to rapid development of functioning of the conditioned reflex, whether it works alike on all ages of and types of children must be questions merely raised for further investigation.

Moreover with objective methods one looks for objective results. In very few of Krasnogorski's experiments do we have statements as to the number of trials necessary to develop the mechanisms.

In one instance he states that after sixteen stimulations a memory reflex was developed (99, p. 21). No statement is given as to the amount of time between these stimulations. If it was even three minutes, the shortest interval he mentions anywhere, the experiment occupied forty-five minutes. This is a period of such a length that for the young child the amount of fatigue in the later stimulations probably almost balanced their impression value. If the experiment was carried on in two or more sittings fatigue may have been avoided but there is no statement to any such effect. At any rate the results would probably be different if achieved at one sitting or if the series was divided and knowledge of the condition should be made available.

Throughout Krasnogorski speaks of hearing "analysers," motor "analysers," visual "analysers" and their functioning which gives specificity to the conditioned reflexes. We have seen his physiological explanation of these functioning entities. Need we accept it? A less mystical explanation seems to lie in a more natural application of the theory of cerebral reflexes in its essential form. A stimulus is applied to some one sense organ. The excitation passes along neural paths into the brain and irradiates into conjunctive paths. Then a second excitant passes in and irradiates from the taste paths to those of motor type leading out to muscular and glandular innervation of the throat, lips and salivary glands. But part of the irradiation from the motor region has sped along the nerves leading towards other centers including the one already stimulated by

our first excitant. This has been doubly used and has a stronger tendency to function a second time than a nerve conductor strengthened by usage only once. After a number of repetitions this path, this habit or association is strong enough to function immediately when only the one stimulus has been given. This is a conditioned reflex pure and simple, or in terms more clearly psychological it is an association, arbitrarily determined, between some one sense excitant and a motor innervation. This is the simplest type of learning.

The persistence of the conditioned reflex becomes in psychological terms the retention of the association. Its degenerescence or dissolution becomes unlearning.

The inhibition of the conditioned reflex is the neural restraint of one reflex by the simultaneously working effect of a second reflex as strong or stronger.

Where the conditioned reflex has been formed upon the memory of an antecedent stimulation we have again the simple association only here it is the image of the sensation or even probably, in the later part of the experiment-series, the sensation of cessation of stimulation which is associated with the motor cortical center. May it be supposed that if the image functions as the excitant a greater number of applications of the stimulus will be necessary, due to the greater vagueness of the image, than in a simultaneous association? May we also assume that if the series needed is not longer the association is rather with the cessation of the stimulus?

The mere fact that Krasnogorski finds that the so-called "memory reflex" does develop as easily as the usual co-temporary reflex rather indicates that this suggested interpretation is the correct one.

The analysers now become the *different* paths of conveyance of sense impressions: specificity of reaction

means sensory discrimination. In the very young child it will be hard for us to tell how far this approaches the physiological limen of sensory discrimination and to what extent it is Seashore's (152) "cognitive" discriminating limen, higher or less fine than the physiological limen due to changes in interest, attention, effort or disturbances. In so far as it is dependent upon cognitive abilities one would hesitate before using it, as Krasnogorski suggests, to map out analgesic or anæsthetic centers unless the mental development of the child has been carefully ascertained beforehand. Moreover the sensory equipment must be examined before we can be sure the cortical mechanisms are getting the proper sensations to differentiate. No reference to any such preliminary study is made by Krasnogorski.

The conditioned inhibition, or as he called it, "lading and discharge," may be explained as a neural association with an additional link in it. The cutaneous stimulation, for example, functions through no direct path to the motor center but is associated with an auditory excitation and the auditory excitation arouses the motor response. Neither one functions alone because in all probability it is the *reception* of an auditory stimulation by the organism when in a certain attitude produced by the cutaneous stimulation plus the memory of stimuli, i. e., a state of active attention, which makes the auditory stimulus a sufficient excitant of the reaction. Can this be shown objectively?

Thus we see instead of a series of new "mechanisms" merely the objectively studied results of a behaving organism. The Pavlov and Krasnogorski reflexes must, as Hough (83) has pointed out, be differentiated from the unconditioned reflex as a separate type of reaction and not be considered merely as an unconditioned reflex

in process of formation. The conditioned reflex is learning, association, habit formation. As such the possibility of its formation is a so-called conscious one, functioning through the cerebrum and by means of the so-called association centers.

The validity of this assumption of the conditioned reflex as a functioning of the neencephalon is confirmed by Burnett's (22) work on the decerebrate frog, which he found could not *learn* to escape a labyrinth but performed unconditioned reflex acts.

The observations of Edinger and Fischer (42) regarding the anencephalic child who lived a purely vegetative existence for three and a half years but never showed any learning ability again corroborate, and in a more striking way, the same interpretation of the conditioned reflex.

To what extent we may class the formation of a conditioned reflex in the same group of mental processes as the verbal associations it is difficult to state. In the verbal association the stimulus may be the same as in the conditional reflex, may vary similiarly in intensity and complexity, may be just as clearly or indefinitely sensed, causing the verbal reaction which outwardly differs from the feeding reactions only in that it involves more varied motor adjustments which are not so fully developed at birth.

The conditioned reflexes as we commonly conceive of them are undoubtedly more direct, and have fewer intermediate steps in their development but how many less and the relative amount of functioning necessary to form an association of either type can only be determined by further experimentation.

The claims made by Krasnogorski for his method and findings are so great that a verification or disproof

of them seems to be demanded from the standpoint of both experimental and clinical psychology. If the method is so easily and accurately applicable and allows of so many variations, it opens up to the experimentalist a most promising avenue of attack upon the infant mind. If it differentiates with such accuracy the defective and pathogenic child while he is still young enough for preventive education and corrective therapy to have a chance, it will revolutionize our present clinical methods.

CHAPTER V

METHODOLOGY AND TECHNIQUE MODIFIED FROM KRASNOGORSKI

SINCE an extensive and at the same time intensive study of all the processes outlined by Krasnogorski would involve a problem too voluminous to be handled with facility and coherence, the selection for preliminary study of the processes which seemed most significant became necessary. As the formation of the conditioned reflex is basal to all the other mechanisms, it, naturally, would be included in any group selected. Then, as the fatigue incident upon the development of the conditioned reflex would be apt to interfere with the successful development of any other mechanism, the further experimentation with any one child was postponed until the following day. But it then became necessary to ascertain the retained functioning ability of the conditioned reflex after the twenty-four hour interval and this consequently was a test of memorial regression or saving through retention of the learning of the previous day.

With only these two processes in mind, after some practice experimentation upon available babies, seven idiots at the Massachusetts School for the Feeble-Minded, located at Waverly, were studied. Due to interesting differences in functioning of the reflex after the 24-hour period, 48-, 72- and 96-hour intervals were also tried. No apparatus was used for recording the mouth opening and swallowings but the movements

were observed and immediately recorded by the experimenter who felt in this series that her mechanization of the procedure of the experiment was not sufficient to enable her to handle with ease the apparatus as well as the child and the food.

The results obtained by this series of experiments will be discussed in their proper connection later.

Next, in order that the acquaintance with the use of Krasnogorski's work might not be in one direction only, a long series of observations was made upon one child. This was a boy of five who lived near enough to the laboratory to be able to come regularly, in spite of bad weather, and whose parents recognized, because of their own University training, the value of such studies and gave hearty co-operation. Recording apparatus was used with this child throughout the series.

The results obtained here showed the need for extensive study of each process to determine the significance of variations in its functioning as well as to determine the probable variations among children of different ages, abilities and capacities. Consequently it was decided that the best method of attack was the study of a few of the processes upon a large unselected group of children.

The processes finally selected for study in the large group of children were the following:

First, the formation of a simple conditioned reflex, or, in other terms, the formation of a sensory-motor association.

The Pavlov school has shown that any stimulus may be made the effective stimulus for the functioning of a reflex naturally conditioned by some other stimulus. Such a stimulus should be as little offensive as possible unless we are interested in a comparative study of

pleasant and unpleasant stimuli, and it should be as simple a stimulus as possible so that the response in different children may not be due to response to different parts of a complex stimulus.

Krasnogorski usually used cutaneous or auditory stimuli with the child lying blindfolded. Of course, one rather naturally reasons that with the child, as with an adult, the position and the bandaged eyes would soon suffice to develope an Einstellung of expectation of being fed. But it is highly probable that these factors remained constant throughout the experiment and the condition persisting through periods of non-feeding soon lost whatever stimulating power it may have had, except that it possibly functioned as a stimulus arousing a state of expectation or alertness.

One question presents itself, however. The author tried this method with the first three children studied. Case A was a defective child, almost idiotic and five years old. He allowed the bandage to stay on five minutes, then began to fuss and tear at it. His hands were held down by the nurse, attendant on him, until the third feeding was made at the end of 6′ 15″ but even that did not distract his attention and he began crying in earnest and stopped only when the bandage was removed.

Case B was a five-year-old normal child. He was told to "lie there quietly" and "wait a little while," when after the second feeding he began pulling at the bandage. This kept him from fussing for two minutes then he announced he didn't "want to stay there any longer."

Case C was a normal child of fifteen months and she cried violently and tried to take the bandage off as soon as she had finished tasting the first bit of sweet

chocolate given as the stimulus.

Krasnogorski himself observed in his earlier work that crying interfered with the conditioned reflex and that renewed experimentation should be delayed until the child was once more quiet. This statement may account for some of the prolonged intervals (25 min., 15 min., etc.) that he mentions in his later articles. But from another point of view how much better it would be to avoid entirely any such emotional change and the consequent uncertain but probable modifications in the development of the conditioned reflex. This can easily be done by making the application of the bandage itself the conditioning stimulus. The interval between excitations is then a so-called "filled" one and may be devoted to play or to other experiments with the same child.

This method was the one used in all experiments reported in this study. The bandage was applied by gently sliding it down over the child's eyes from above, with a slight but firm pressure of one finger over each eye, thus inducing the most certain exclusion of light and then the bandage was kept in place 20″. In the eleventh second the child was fed a bit of sweet chocolate and the bandage was removed at the end of the twentieth second. Then the child was allowed to sit up and was kept busy with other tests for the interval that must elapse before the process was repeated. The lying down was itself kept from becoming the conditioning stimulus by frequently lying the child down in the intervals between experiments. The three-minute interval was used. That is, it was three minutes from the initiation of any one stimulation until the beginning of the next stimulation.

Several factors determined the choice of these time

intervals. Ten seconds is the least period of excitation used by Krasnogorski and it seemed well to use a period that would give a basis of direct comparison with some of his work. This shortest period was chosen because of the probability that a finer, more extensive series of numerical results could be obtained (in terms of the number of applications of stimulus necessary to evoke response) if the stimulus were less and hence less effective in any one application.

The three-minute interval was chosen instead of the five-, ten- or twenty-minute interval in order to expedite the development of the association. Probably here, too, a finer gradation in number of trials necessary to obtain a positive reaction would have been obtained if a longer interval had been used but this consideration seemed outweighed by the following factors:

1. The need of finishing each day's work with the child before fatigue was sufficient to play an important rôle in results obtained.

2. The need of finishing the work with each child in a small enough number of days so that he would not lose interest in the general situation.

3. The need of developing any standards that might be evolved in an expeditious procedure which would be short, direct, specific enough for clinical use in case Krasnogorski's claims of the diagnostic value of the method were confirmed.

The average length of time a child was to be used was set at one-half hour. This allowed for the initial procedure and ten repetitions if that number proved necessary. In case more were needed the procedure was continued at twenty-four-hour intervals until the desired result was obtained. The child was considered to have learned to associate the bandage with the feed-

ing of chocolate when he twice in succession opened his mouth for the chocolate before the ten seconds preceding the stimulation by chocolate had elapsed.

It was felt necessary to use the two responses because sometimes one response might be observed and recorded and considered an opening of the mouth in anticipation of food when it was really a slight yawn, a cough, a deep breath, or a laugh.

The appearance of two consecutive positive reactions ended the work for that day.

Twenty-four hours later the second process was carried out. This was the memorial functioning of the association and it was proposed that it be tested by the Ersparnis Methode—calculating the number of trials less than the number used in developing the association that were necessary to cause it to function in the same manner. Unfortunately any one memory interval chosen arbitrarily is not apt to be the most efficient interval. The twenty-four-hour interval was one that had best claims for investigation, however, from our standpoint. It allowed, as do any of the day intervals in contrast to the part-day intervals, a second experimentation with the child under conditions of feeding, rest, and play most nearly approximating those of the previous work. It also is an interval short enough to be used possibly in the clinical study of children, and, as the difficulty of getting children who are not under one's immediate control seemingly doubles with every additional day over which the experimentation must extend, it seemed practically worth studying. Moreover, it may allow for some comparisons with recalls of reagents on other problems where the interval of delayed recall most frequently used is one of twenty-four hours.

As soon as the associative functioning was re-established in the child the third process was begun. This is what Krasnogorski called the inhibition and degenerescence of the conditioned reflex. The bandage was placed over the child's eyes just the same as when the association was developed but this time the child was not fed. This was repeated until the anticipatory mouth openings and swallowings were entirely absent in two successive trials. The association was then considered to be inhibited or unlearned to a point below the functioning level. From an objective standpoint we can not say whether it was really an inhibition that was developed or merely a fading out of the association previously formed, but the term "unlearning" indicates merely the absence of a response while the term "inhibition" more frequently means the restraining action of an additional, newly-introduced factor, so it seems better to leave the latter term to be applied to the more complex Krasnogorski process described in Chapter IV, and to designate the third process used in this study as "unlearning."

The number of trials necessary to effect the unlearning were in some cases too many to allow of full development in half an hour. The question arose whether here as on the learning process the work should be suspended until the next day and after due consideration was answered in the negative. Here we are dealing with a process more apt to be unpleasant to the child than pleasant. In the learning he has been fed every time with a bit of chocolate and learned to expect it, here he expects it and doesn't get it. The probabilities were that if taken home in that state he would be apt to refuse to come back the next day and the work would remain unfinished. Also there was the

probability that the effect of the partially effected unlearning might be strengthened in some and weakened in others during the twenty-four-hour interval so that we would have to deal with an unknown factor in evaluating our results.

Feeling that these considerations were sufficiently important to make it worth while risking fatigue the experiments were continued until the desired results were obtained, although every effort was made through varied plays and games and story-telling to decrease the fatigue factor as far as possible.

For a test of reassociation, the fourth process studied, immediately after a three-minute interval had elapsed after the completion of the unlearning, the stimulus was repeated but this time the child was fed. This was repeated until he again reacted positively twice in succession.

A saving of several trials was effected in the following manner: The first time the conditioned reflex functioned when it was being memorially tested on the second day the child was of course fed but when on a successive trial it functioned similarly the child was not fed. Thus although the trial gave us our positive reaction indicating that the association functioned, yet it also became the first in our series of trials for unlearning. Likewise the second of two successive trials indicating complete unlearning was used as the first of the series to correct the unlearning by giving the chocolate. Also, no preliminary record was considered necessary as indicating the quiescent state of the child other than the foreperiod of the first actual trial.

With this procedure carefully carried out one has a quantitive expression of the ability of the individual (a) in associative learning, the more rapid the learn-

ing the fewer the number of trials required; (b) in memory or forgetting, the fewer the number of trials the greater the retention; (c) of the inhibitory effect of a change in conditions and hence of unlearning or adaptability, the adaptation being greater the fewer the number of trials required; and (d) re-association or re-learning, this being less difficult the fewer the number of trials required.

The objective method of recording used was a slightly modified form of that described by Krasnogorski. A small receptive disc of a Marey tambour, attached to a long rubber tube, was placed under the hyoid bone and over the thyroid cartilage. This disc passed through a slit in a strip of linen and was held in place by this strip being wrapped around the child's neck. The rubber tube connected with a recording disc which recorded all the movements of the throat and lower jaw upon a kymograph. This recording apparatus was kept in place throughout the experiment, a procedure rendered feasible by having the rubber tubing plenty long enough to allow the child free movement without his disturbing the adjustment of the recording end.

The time was likewise recorded on the kymograph by a Jacquet chronometer and an electric indicator controlled by a telegraph key made it possible to record the actual moment in which the food stimulus was given, as well as the presentation of other stimuli.

Another point that had to be considered was the method of handling the child himself. This method begins logically with the manner of handling the community in order to obtain permission to use the children. A wrong attitude developed in the parent may not always bring a refusal of the use of the child but may

instead bring such comment and criticism within the family that the child feels the attitude and reacts accordingly towards the experimenter.

Of course with the low grade defectives there was little trouble. They were under institutional care and consent was readily given by Dr. Fernald when permission to use them was asked. They themselves were too low-grade to seem much influenced by even the petting and caresses of their nurses and being in an institution were accustomed to the presence of little-known faces. Care was taken to visit and study all of them, though, before experimenting began and with the two of highest grade quite a play intimacy was developed.

With the normal boy Partil who was used for some time the intimacy was developed through two trips to his home before he was asked to go to the laboratory, but there he made up readily and consented at once to come, although his parents thought this a little unusual as he was rather timid. That the relation was perfectly normal is shown by the fact that the second time I called at his home he was taking a nap and on awakening and hearing that I was there he brought his shoes and suit in and asked me to help him put them on and button the buttons.

In the attempt to use a large group of children from average homes the situation is somewhat more difficult. Consequently the experimenter studied first of all the village habits and conditions before attempting to gain permission to use the children.

The work was done in a village of about 400 inhabitants. (1910 census says 241.) The village is suburban to one of our large eastern cities and has splendid railroad service, rendering the metropolis easily accessible for business and recreation. It is also on a State

Road which gives rise to heavy automobile traffic and makes possible a goodly amount of automobile trade. It has a community composed of a large percentage of professional men, commuting to the city, a still larger group of railroad employees of middle scale—engineers, conductors, electrical staff, etc., due to the village being the terminal for all suburban trains, and a new and growing percentage engaged in cement-block making. Besides these there is a group of some twenty to forty men employed by the leading business concern of the place, a wholesale and retail flour, feed and lumber mill and coal yard. The most significant fact perhaps is that there are in a literal sense no dependents in the community. Some years ago a number of good ladies of the place organized a Dorcas Society and sewed for the poor, only to find that there were no poor to accept the garments when they were made.

Of course there are those who are poor, but as each little house usually has its own garden and as almost every one keeps a few chickens, smaller wages suffice and the independence and frugality of these poor not only makes charity impossible but also shows that they are normal individuals even if not well to do.

No one group, or social level, was used in this experiment but re-agents were drawn from all as far as possible. A couple of rooms near the center of the village were rented and starting with children nearest at hand they were sought in an ever-widening circle until practically all resources were exhausted. Here and there a child was skipped because he was away when that part of the village was covered or because he lived so far away from all other children as to make the trip, proportionately to the need of using him, rather a proceeding not worth while.

The majority of the people dwelling there were known, at least by name, to the experimenter and through them the rest were easily met or in many instances self-introduction worked informally and well.

The experimenter studied first of all the village habits. Like true German housewives, since they lived in a community where German traditions if not German blood flourished, the morning hours were busy ones for the most. Housekeeping, cooking, gardening, cleaning, washing, made morning calls unsolicited and painfully formal. Early afternoon meant children and many mothers napping, but about four o'clock the broad porches and big or little lawns were in general use. Neighborly chats and semi-social trips to the grocery store or for the mail showed the village as a place full of real live people, but as most of them were financially situated so that they did their own cooking, they were not apt to be far away for a long time, for the man of the house here receives due consideration of his labours and needs. In the evening coolness the whole little family was apt to jaunt out to some farther point —the moving picture, the lanes lined with berry bushes or the neighboring, more metropolitan, village.

Consequently the time between four and six, not too near six, was chosen as the time to solicit the loan of children. Beginning with the children of the "best citizen" was avoided. Instead I chose to ask the wife of the chauffeur of one of the best citizens to lend me her three babies. The fact that I'd take them all at once, for a whole morning, two days in succession, appealed to her gossip-loving, care-hating mind, and she consented. By the next morning the news of the choice as well as the fact that I had asked her to help me get a complete list of other children was over the whole vil-

lage "circle," for which she helped set the pace. During the two days I used these children I visited other parents in that section, securing their consent and letting them name any two days that suited them best, explaining with emphasis the importance of the second day, and getting acquainted with the children. This was not difficult. The news of toys and games and of pennies to spend for candy before one came home scored every time.

Whenever there were two or three children under eight in the family I tried to use them the same days and, if there were only one or two, other children whom they knew were taken the same days. I found I could handle three in a morning and this number was the most satisfactory. If I had only two, one was left to play alone while the other was being used. If there were three, two played together, although if they were very young I had a young girl come to assist by taking care of them. If more than three were used the experiments ran too near breakfast period on one extreme, and too near lunch time on the other, so that satiety and hunger in more extreme forms were introduced.

Calling for the children myself gave me a chance to get acquainted with them more fully while on the way to "my house." Once there, on all pleasant days the children helped take the playthings out on to a large shady corner porch connected by a glass door with the room used for experimenting. In bad weather they played in an adjoining room. Sometimes an older sister came along. This usually simplified matters but the permission was always offered as a great privilege and was naturally always so regarded.

Then the children usually wandered around in the

room while I put up windows, smoked the kymograph paper, wound up the kymograph, etc., and prepared record sheets. They usually asked what the kymograph was—what it was for, and even if they didn't I always explained—explained so as to form an association fully dissociating its slight noise from the main experiment—that I ran it to make the room cooler and keep flies and mosquitos out. Then I allowed it to run so that they would become accustomed to the noise. The rest of the apparatus was never taboo—any questions and curiosity were eliminated *before* we began experimenting.

The next question was how to attack the work itself. Here a consideration of the fundamental nature of the child gives us as an attribute most likely to form an adequate basis for handling him, his instinctive love for play. So our whole procedure became a game. The children were told that I wanted to play something with each one alone and so I'd take Marie first and then James. The others could play house, tea party or what they wished on the porch. Schmitt also recognizes this for she writes: "With him (the child) the motive most conducive to natural reaction, uncomplicated by disturbing emotions, is the play motive" (149, p. 13). Two factors had to be watched, however. The toys must not be given in such profusion as to make a child lose all interest in what might be going on indoors and the play indoors must be made sufficiently attractive to make him eager to come back a second day.

The chocolate stimulus itself was quite effective in this way and then if there were any things a particular child disliked in the secondary tests they were left until the second day. Also on our way home we usually

stopped at the corner drug store and each youngster had an ice cream cone. By the time he had finished this he was home and the recollections of any unpleasant details of fatigue were lost in the anticipation of another such treat. Also the fact that I treated their children in such an unnecessary fashion made many of the mothers consider themselves under more heavy obligation to let me have the children the second day.

The most conclusive proof I can state to the effect that what extra trouble there was was worth while is that no mother refused to let me borrow her children, although in one case the father had to be consulted before she'd say yes. Moreover, of all the children tested one day, none failed to come the second day except one little boy that I refused to take because he was ill. The rest all wanted to come back again, offering to come with other children, to help me, to put the toys away or even in several instances declaring they wanted to come and live with me.

During the two-and-a-half-minute intervals in all of these processes a definite procedure was followed. Bit by bit the Binet tests and those additional tests necessary for the Point Scale evaluation, were given. The Healy Construction Puzzles A and B, the Healy Foal and Mare and School Puzzles, the Goddard Adaptation Board, and Goddard modification of the Seguin Form Board were also used. Also each child was measured for standing and sitting height, weight, dynamometer measurement of grip with the right and left hand and lung capacity as measured by the wet spirometer.

The intelligence of the parents was also rather pointedly observed during the visits to the homes, which also gave significant facts regarding each child's environment. These estimations of intelligence of the pa-

rents as average or not were corroborated by appeal to the family physician of the village, and in still doubtful instances by appeal to the leading business man of the village who was in financial touch with practically all of them.

One requirement that is harder to fulfill when working with children than it is when working with adults is that of uniform verbal Aufgabe. Even though the task itself may be uniformly explained or directed the child cannot and does not (nor need he) understand the purpose as even an untrained adult does. He asks questions and they must be answered; he raises objections, they must be met; he modifies his procedure to suit his own pleasure. The control of all this must not be one of forcing into a narrow and fixed groove all responses so that they may be used but in a quick adaptation of non-essentials which are kept before the child while the process under observation is tested under unmodified conditions. One may have to pet, coax, bribe, or even with some children threaten a little but I write this last word rather hesitatingly. In most children threatening of any sort upsets the normal poise. Now and then one finds, however, the child who may be threatened by the idea of how badly he would feel if he failed, if some one else, a concrete person, did better than he. Or one finds the child who may need to be told that we won't need him unless he tries harder. But usually the work should be held as a privilege and open choice on the material or activity to fill the rest intervals will lead the child to feel he is perfectly free in the whole procedure. One of the chief advantages of this method of conditioned reflexes is its utter lack of any need for verbal directions, the child formulates for himself his response to

a situation.

Besides the kymograph record of the movements, the observed movements not only of mouth and throat but of the whole body, any unusual disturbance and any verbal or behavior reactions were noted immediately after each trial.

The extra tests, which consisted of all the more generally used clinic tests for mental diagnosis that are applicable with such young subjects, were given in a standard way, the one adapted by the experimenter after five years of experience with such tests.

In using the Binet Scale the Goddard (58, 60, 62) Revision was used throughout and supplemented, as is done in the Vineland Laboratory and the Massachusetts School for Feeble-Minded, by the Binet-Simon series of six easier tests for idiots (57).

The Point Scale was used as accurately as possible according to directions given in the article in print at that time (190), as the Yerkes-Bridges (191) book on the Scale had not then been issued from the press. Where the questions in the Binet and the Point Scale were similar but varied in difficulty of presentation, the more difficult form was given first and if that failed the less difficult method was used, credit being given accordingly.

The Healy Boards were all four presented to the child one after another with the directions suggested by Healy and Fernald (75). Minute records were not made of the errors but merely a record of the total length of time and the type of procedure, although if Schmitt's (149) standardization had appeared before the experiments had been made I would undoubtedly have followed her suggestion and recorded the time on the triangles in the Foal and Mare Puzzle sepa-

rately.

The Adaptation Board was used as Goddard (63) suggests but with these modifications and additions. Due to the brief, indirect directions given it may be possible that a child fails on the first turn because of lack of or rather misdirected attention. Hence, if a child failed on the first trial but succeeded on the second, another trial was given on the turn from lower left to lower right. Then several new moves were also added to the board's use. These are ones suggested but not standardized by Goddard (63, p. 188). The third turn, or first added turn, was from lower left to lower right and on to upper right without stopping. The fourth turn was from upper right to upper left to lower left to lower right without stopping. These additions allow of a wider use of the test and finer gradation of ability.

The Form Board was used as Goddard (61) and Sylvester (168) suggest. It was given three times the first day and, on the last nineteen children, three times on the second day. This usage on the second day suggested itself at that point as another corroboration of the memorial regression of learning. This board, as well as all other of the tests were presented to all the children used, regardless of any doubts the experimenter had, due to the age of the child, as to its ability to perform the required tasks. The time was recorded with a stop-watch and the child urged to hurry and make speed a part of his goal-idea.

Height standing and sitting were measured in millimeters and expressed to the nearest centimeter.

Weight was measured in kilograms to the nearest tenth.

Grip was measured in kilograms to the nearest tenth

upon an adjustable Smedley dynamometer.

Lung capacity was measured by the wet spirometer and expressed in liters to the nearest hundredth.

With exactly the same procedure another group of such children as tested normal or nearly so by the Binet and performance tests but who were, because of heredity or a-social reactions, suspected of mental defect was studied. These children, seven in number, were inmates or observation cases at the Massachusetts State School for the Feeble-Minded.

Also, two children were accessible who had been clinically studied and diagnosed as not feeble-minded but as "normal on all tests." These children had been taken to the clinic because of suspected defect due to lack of ability to get along in school.

CHAPTER VI

PRELIMINARY EXPERIMENTS AND THEIR INDICATIONS

THE preliminary investigation of the use of Krasnogorski's method was twofold. First a study was made of seven young defective children who were available. It seemed feasible that a method whch could be applied with definite results upon such low-grade children would be applicable without difficulty upon normal children; also, if Krasnogorski's statements were true, the conditioned reflexes would here be obtained only with greater difficulty than upon normals and hence their development would allow more experimentation with the method itself.

Secondly, an extensive study was made of one child, endeavoring to develop a number of the various mechanisms, but here again primarily with the aim of studying and understanding the method.

The work upon the seven low-grade defectives involved two processes, learning and forgetting.

Although the method used with this group varied somewhat from individual to individual, due to the fact that the procedure was one arbitrarily determined yet the results seem worth presenting in detail, chiefly because they form the basis for the definite procedure followed upon the larger group of children used in the later experiments.

The plan begun with was to leave the child lying

on a low couch (all but two of the seven studied spent their whole day lying down or tied in a chair) and to apply the bandage as previously described. Then after 5 seconds to stimulate the right arm above the elbow for 10 seconds and in the sixteenth second to feed a bit of sweet chocolate, to be exact, a piece one centimeter square.

The first modification came in the food stimulus. Three of the seven children refused to eat candy and were accustomed to liquid foods only. This fact was ascertained beforehand. Each was then experimentally fed a bit of chocolate and when it was definitely refused a new stimulus, sweetened water, was adopted. Two of them took this readily from a spoon but the third refused this when made from white sugar and took it only when it was made from brown sugar to which he had become accustomed. It was thought better to use this rather than honey, which was used by Krasnogorski, as honey is not only an unknown taste quality to some children and not always liked when first tasted, but the use of it is apt to lead rapidly to a feeling of satiety, at least such is the report of a number of older persons, and our stimulus should be just sufficient to evoke with the sensation a pleasant affective toning making the subject desirous of more.

The next modification was that regarding the use of the bandage. It seemed highly probable that these defective children were so low grade that they might easily be persuaded to lie with the bandage over their eyes throughout the experiment. Experience proved the opposite. Their negative reaction to the bandage, if violence of behavior be a just criterion, was far greater than their positive reaction to the sweet fed to them. Two of them cried out at first when the

bandage was put on and all of the others, even the very lowest grade ones, tried to get it off after they had been fed and lain a little while.

Since the difference in reactions is marked in a qualitative as well as a quantitative manner it seems best to describe the results by the individual cases.

1. Leitha was a little girl, aged 55 months, born in this country of English parentage. Peculiarity manifested itself through bumping of the head, crying and convulsive muscular attacks, etc., between the ages of one and two. She walked at 25 months but her gait is still straddling and uncertain. She has so far not formed any habits of cleanliness, does not talk and has to be fed with a spoon. Indeed if she is hungry and food is on the table within reach she does not know enough to take it. She does not seem to recognize the nurses who are with her the whole time. She has been diagnosed as a low-grade idiot with a mentality of less than one year. She has not improved since these experiments were made.

I began trying to develop a conditioned opening of the mouth through excluding light by a bandage with a cutaneous stimulation upon the right arm above the elbow. At first she was rather restless and grew markedly so if the bandage was kept on after she was fed and had eaten the candy. Ten trials were given the first day without the slightest indication of an association having been made. On the next day she lay quiet for the first two trials and on the third trial opened her mouth, grinding her teeth and said "eh-ah" before she was fed. This was repeated the fourth time, so the conditioned association had been developed in 14 trials.

The conditioned reflex was stimulated and functioned

without defect six more times that day, marking 10 trials in all, then the child was allowed to rest. The reaction was, however, to the application of the bandage before cutaneous stimulation was applied and so during the rest of the work upon her the bandage alone was used.

Twenty-four hours later the procedure was repeated. The first trial she was quiet but on the second trial she at once began smiling, making noises and opening her mouth. This happened on three successive trials.

After another forty-eight hours, when the process was again tried, there was no response upon the first two trials but upon the third trial she opened her mouth after nearly ten seconds of stimulation by the bandage, similarly on the fourth trial, while on the fifth trial she began smacking her lips as soon as the bandage was put over her eyes.

She was allowed to rest 4 days and then the experiment was repeated. The child was not in good condition for the work. She was very restless and continually falling into the masturbating movements characteristic of idiots, grinding her teeth at the same time. She seemed, however, to react positively upon the seventh trial and this is probably accurate as there was no difficulty in deciding that she had not reacted positively before and as this seeming reaction was present in three succeeding trials.

Twenty-four hours later six trials were necessary for functioning and this functioning continued through the four succeeding trials given on that day.

Forty-eight hours later 4 trials brought the required reaction.

With Leitha, and on the others of this group as

well, an effort was made to keep the number of trials from day to day at either five or ten. This proved disadvantageous and was not tried in any later series. One of the disadvantages is the increased difficulty in calculating the number of trials necessary for memorial functioning and saving. To do this we must count all the trials after the proper functioning on a given day and add them to the number required for functioning upon the next day in order to get the full series.

If we do this with the data obtained upon Leitha we have the following results:—

For Learning the number of trials required is 14.
For Memory the number of trials required is 9 after 24 hours.
For Memory the number of trials required is 6 after 48 hours.
For Memory the number of trials required is 9 after 96 hours.
For Memory the number of trials required is 8 after 24 hours.
For Memory the number of trials required is 8 after 24 hours.

If, however, we disregard the trials given after the conditioned reflex functioned on a given day and count only the number of trials necessary to re-establish it the next we have the following number of trials required:

After an interval, since last functioning, of 24 hrs.—3 trials were needed
After an interval, since last functioning, of 48 hrs.—4 trials were needed
After an interval, since last functioning, of 96 hrs.—8 trials were needed
After an interval, since last functioning, of 24 hrs.—6 trials were needed
After an interval, since last functioning, of 48 hrs.—4 trials were needed

These figures vary much more nearly as the time factor, and, although any deduction is merely a surmise, seem to indicate that the habituation of an idiot child to a process newly acquired is not greatly helped by increasing the number of times the habit is exercised any

one day but that functioning at short intervals is more important. This agrees with the conclusions reached by those studying the relative value of grouped and distributed repetitions for learning but it is also probable that the effects of fatigue may be more marked here than they would be in normal subjects or adults and so render the last trials on the day of learning relatively weak in effect.

2. Pasha was a boy about 50 months old, born in America of Armenian parentage. He is a helpless diplegiac, not able to move much. He is not clean, cannot feed himself, seems to be able to understand a few words. Diagnosed as an idiot with a mentality of one year. No improvement since.

Since he eats only liquid food he was given sweetened water. With him I used bandage on 5″, followed by stimulation of upper right arm with camel's hair brush for 10″, then feeding in the sixteenth second.

He developed the association between bandage and feeding in the fourth trial but from the fifth trial on did not begin reacting until brush touched the arm.

The results obtained with him were as follows:

Length of interval since last previous trial	24 hrs.	48 hrs. 1st trial	48 hrs. 2nd trial	96 hrs.
Number of trials needed on day of new trial	3	3	4	2
Number of trials of over-learning that had been given on the day of last previous trial	4	2	2	1
Total number of trials needed for functioning	7	5	6	3

Learning to associate bandage and feeding took five trials while learning to associate bandage, brushing the arm and food took six trials. The memorial functioning took a varied number of trials in the different length intervals and even in two different trials after the same length intervals.

3. Ahil was a boy 51 months old, born in America of Russian parentage. He was a seven-months baby, is spastic in both legs and rather microcephalic. He is unclean and does not feed himself. He has been diagnosed as an idiot with a mentality of between one and two years of age. He showed no signs of improvement in the year between the time of the experiments and his death.

Here, also, an attempt was made to develop the discriminating reflex to the cutaneous stimulation. During the ten trials on the first day no response to the situation developed but on the second trial the next day the child reached up and tried to pull the bandage off after he was fed. He had not tried to pull it off before. This happened throughout the rest of the trials that day. There was no reaction to the bandage when applied nor to the cutaneous stimulation.

After twenty-four hours he took the bandage off on the second trial. Two days later he removed it on the first trial, and 4 days later and again after 4 days he removed it upon the first trial. Throughout he showed no other reaction to the situation. After the experiment was finished it seemed advisable to examine his skin sensitivity with the result that we found him almost completely anæsthetic and even analgesic, except on and around his lips, to such minimal stimuli as were being used in the experiment. The motor reaction of removing the bandage is, however, as good

as any food reaction for the study of the learning process and shows here the fumbling but sure adjustment of even a faulty organism to environmental conditions. Summarizing, we see that the association developed in 13 trials while memorial functioning was as follows:

Length of interval since last experiment	24 hrs.	48 hrs.	96 hrs.	
			1st trial	2nd trial
Number of trials needed to establish functioning on day of re-trial	3	2	2	2
Number of trials of over-learning given on day of previous learning	7	2	3	3
Total number of trials given	10	4	5	5

Here again we see that the regularity of the number of trials given at each sitting interferes with a direct study of the number required for actual retention and probably is in excess as the association is found to function in the first two trials after 2- and 4-day intervals since last functioning.

4. Jorsi is a boy 60 months of age, American born of Italian parents. He is a swarthy youngster covered all over with fine black hair. He is rachitic and the sternum protrudes sharply. He has a slight lumbar scoliosis and enlarged epiphyses of the wrists, elbows, ankles and knees; has also umbilical hernia and large tonsils and adenoids. He is clean and feeds himself with a spoon. He can walk by pushing against a chair; plays real nicely and with good coördination with small

objects. Diagnosed as an idiot with a mentality of 1 ^{2} years by the Binet. (No marked improvement before death, which occurred recently.)

With Jorsi, also, an attempt was made to develop the conditioned reflex to a cutaneous stimulation. At first he gave no reaction except to try to get the bandage off even before the feeding, then on the ninth trial he lay quiet but took the bandage off at once after he was fed. This happened on the tenth trial also. After twenty-four hours he removed the bandage on the second trial but not until the third trial, or thirteenth in all, did he open his mouth before he was fed. Even then this reflex functioned irregularly, being firmly established only after the nineteenth trial, although the reaction of removing the bandage functioned regularly after the twelfth trial.

After another 24 hours he removed the bandage on the first trial but opened his mouth only upon the second trial.

Forty-eight hours later both mouth opening and removing the bandage came at the correct time in the first trial. One point is to be noted, however. At no time was there any reaction to the cutaneous reaction as distinguished from the application of the bandage. His mouth opened as wide before the brush was applied as after. He was not suffering from anæsthesia.

Considering the development of the association between food and removing bandage we find establishing it took 10 trials, while that between bandage and feeding took 14 trials, then functioned irregularly but after 19 trials is fully established. The memorial functioning is as follows, the numbers in parentheses being for bandage-food associations, the other for feeding-remove bandage associations:

Interval since last experiment	24 hrs.	48 hrs.
No. of trials given on day of re-trial	2 (3)	2 (2)
No. of trials of over-learning given in previous experiment	7 (6)	3 (3)
Total number of trials used	9 (9)	5 (6)

Jorsi was sick when the time came for a 96-hour trial of memorial functioning and so was not tested after that interval.

5. John was a boy about 36 months old, born in America of Italian parentage. He was reported as "peculiar from the time of birth." He has a cleft palate and hare lip but both soft and hard palate have been repaired. He does not walk or talk but feeds himself with a spoon. Is diagnosed as an idiot with a mentality of 1^{8} by Binet and as probably hydrocephalic. He has since died. No microscopical findings are as yet available upon any of the three who have died but the description of gross physical attributes has not been reversed by the autopsy studies.

An attempt was made to develop an association between cutaneous stimulation and feeding. On and after the fifth trial he reacted to the application of the bandage by opening his mouth with mouthing and sucking movements. After three more trials the mouthing was deferred until the cutaneous stimulation was applied, or the first type of association was formed in 6 and the second in 9 trials.

After twenty-four hours the cutaneous-feeding association functioned on the second and successive trials. After another twenty-four hours the cutaneous-feeding association functioned at once. After 48 hours the bandage-feeding association functioned at once but it took five trials to redevelop the cutaneous-feeding asso-

ciation. After 96 hours more the cutaneous-feeding association functioned immediately. The results are as follows, figures representing bandage-feeding or bandage-open mouth associations being in parentheses:

Length of time since last experiment	24 hrs. 1st trial	24 hrs. 2nd trial	48 hrs.	96 hrs.
No. of trials needed on day of re-trial	3 -	2	5 (2)	2
No. of trials of over-learning previously given	1 -	2	3 (3)	0
Total number of trials inducing the reaction	4 -	4	8 (5)	2

6. Jock is a boy 34 months old, American born of American parents. He had convulsions when about 18 months old, but probably was not normal before that time. What is known of his parents is not very much in their favor, being merely that they have never lived long enough in any one place to have legal residence there. The child is somewhat undersized, does not talk, is not clean in his habits, learned to walk at two years of age but is rather unsteady in his gait. He feeds himself with a spoon. Is diagnosed as an imbecile with a present mentality of about two years.

The experience with the preceding cases led to a modification of our procedure here. No use was made of the cutaneous stimulation at first but an attempt was made to establish the association between the bandage and feeding, the bandage being kept on ten seconds before the food was given.

Only five trials were given the first day as Jock rather

disliked the bandage although he seemed to like the candy. The second day ten more were given but without any reaction before the feeding. On the third day he opened his mouth anticipating feeding upon the second and successive trials, making 18 in all for the learning. Seven more trials were given that day, all with positive reactions, indeed before the last trial, during the play interval, he managed to get possession of my handkerchief, put it on the back of his head and opened his mouth, evidently for candy.

After 24 hours the association functioned at once and was strengthened throughout five trials. Then in the sixth trial for that day a double procedure was tried. Period *a*, the bandage was put on 10 seconds, no candy given, and it was taken off. Period *b*, after 5 seconds it was put on again, and cutaneous stimulation applied at the same time, candy being given at the end of 10 seconds.

Immediately after this first trial Jock took another brush with which he had been punching holes into paper and rubbed his own arm.

In the next trial he stretched out his arm as soon as I put the bandage on and opened his mouth, not discriminating periods *a* and *b*. This happened in the next trial but in the last two trials of that day he lay absolutely without reaction in both periods.

After 24 hours he reacted only when touched by the brush in the second and successive trials. In the eighth trial for that day he developed a new reaction. As soon as the bandage was put on he would stretch his arm out towards me, although in stimulating it, it had always been left in its natural position alongside his body, and then after it was touched he would open his mouth.

After twenty-four hours more, when tried again, he was not very well but seemed to want to go through our game, running to me as soon as he saw me coming. Discrimination seemed lost as he would open his mouth before as well as after the cutaneous stimulation. His own activity, however, showed that some sort of an association was there, for as he sat playing on the couch he put a handkerchief on his head and tried to rub his hand with a pencil, then wanted me to take it, and, when I did, he lay down and opened his mouth.

Two days later no reaction was obtained which lacked discrimination and this correct reaction appeared also upon presentation of the cutaneous stimulation, and to it only, 48 hours after this first 48-hour trial.

7. George was a boy 73 months old, American born of French-Canadian parents. He was a seven-months baby, his mother was alcoholic and has since disappeared, while his father is in a Home of Correction. The child's defect was noticed at one year of age. He began to walk at three years of age. He says a few sounds, feeds himself with a spoon and is clean in his personal habits. Has chronic ringworm of scalp, masturbates, is very nervous and excitable, although usually very good tempered. He talks incessantly in a jargon of his own, tries to imitate sounds but does not attempt a systematic use of even the few words he knows. His coördination of eye and hand is very good, and he seems to notice a great deal in his environment. He has been diagnosed as an imbecile with a present mentality of between two and three years.

The same method was used with him as with case 6. At first he tried to remove the bandage before he was fed but gradually this activity disappeared and after 8

trials he had learned to remove it after he was fed but made no attempt to do so before he was fed. Not until the second day did he open his mouth before the feeding, but it developed in the first two trials on that day, taking twelve trials in all, but on the second trial that day he stopped removing the bandage and only re-developed this reaction in connection with opening his mouth upon the fifth and sixth trials of that day. After that the series worked as if automatically. I would place the bandage on his eyes, his mouth would open, I would give the chocolate, his hand would remove the bandage and then, usually, he would laugh.

After 24 hours this series functioned once. Then an attempt was made to develop the inhibition of the mouth opening unless the cutaneous stimulus accompanied the bandage by the procedure of alternation used and described in case 6, Jock. At first he opened his mouth under conditions of both the *a* and the *b* periods. On the eighth trial he opened his mouth once when the bandage only was used, then closed it, opened it also in *b* of that trial and moved his arm as the brush touched it. In trials nine and ten he did not open his mouth for either condition but as he made no attempt to remove the bandage it seems obvious that he in some way anticipated food.

On the first trial the next day he opened his mouth in period *a* and was not fed, then kept it closed for *b*. On the second trial his reaction was the same. On the third trial he made no response in period *a* but opened his mouth once in period *b*. In trials four, five, six and seven he was quiet during period *a* but opened his mouth during cutaneous stimulation, i. e., period *b*. This shows beautifully the various stages of development of a discrimination or of inhibition to one group of conditions,

mentioned by Krasnogorski. But this seeming discrimination might be due to an established rhythm of no response—response, consequently, in the eighth trial the cutaneous stimulus was given at once, *a* period being omitted, and the correct reaction appeared, while in trials nine and ten the *a* period was used without evoking response which functioned correctly in the *b* period.

After 24 hours the reaction was absent from both *a* and *b* periods for two trials but re-developed in *b* in the third trial with the addition that he would stretch out his arm at right angles with his body and say "la-la-na-na" before the cutaneous stimulation began. This occurred regularly during seven succeeding trials given that day.

After 48 hours there was no response upon a first trial but the series of reactions appeared upon the second and succeeding trials. On the fourth trial, before it was time, he lay down, tried to put a bandage on his eyes, stretched out his arm and opened his mouth. Forty-eight hours later he repeated this same series of acts as soon as I began working with him.

This series convinced me that the mechanism of a conditioned reflex or stimulus-reaction association could be easily developed with even young children, but also showed that the conditions were more complex than had been supposed while the variations in details of method loomed up as innumerable. But before attempting to determine just which points would best repay for a quantitative and detailed study, another series of preliminary experiments, this time a developmental one, was carried out upon one and the same child.

Partil was 65 months old when I began working with him. He was unusually well developed physically for his age but very nervous, given to small twitching move-

ments of which he seemed unconscious. By the Binet tests he tested 7 4, accomplishing in all 34 points. On the Bridges-Yerkes he scaled 32 points. He could not do the Healy Construction Puzzles A or B, nor the Foal and Mare Puzzle without help. The School Room Puzzle he succeeded in completing but with the blocks for the window and blackboard spaces interchanged, nor did he notice this error until it was called to his attention. He learned rapidly, however, and did all of these without difficulty when given a second trial.

He had been accustomed to having the bandage of the receptive Marey tambour around his neck while he played in the laboratory and did the various formboards, also to having the bandage over his eyes at irregular intervals while the kymograph and chronometer, separated from the couch where he played by a screen, were kept running during the whole fore-period.

The reaction to cutaneous stimulation with the eyes bandaged was first developed by putting the bandage on and after 5 seconds starting the cutaneous stimulation, then feeding ten seconds later. This conditioned reflex functioned on the fourth and fifth trials. Then work for that day ceased. After 24 hours the retention value of the association was studied. The first trial found the child very quiet, on the second trial slight twitchings and openings of the mouth were noticed while on the third trial the mouth openings were very marked and breathing was heavy enough to be recorded in the curve of throat and mouth openings. That the child anticipated candy may be seen by his question as to where the candy came from which he answered for himself by associating it with the black kymograph for he said, "Oh, I see, that (the kymograph) turns and it comes out of there (pointing in the top) and you give

it to me." After forty-eight hours the association functioned slightly on the first trial and markedly on the second and third trials. Owing to the child's going away for Christmas work stopped at this point and was resumed after an interval of 25 days.

While I was adjusting the apparatus the first day after this interval Partil said, "I know what that feels like." "What does it feel like?", I asked. "Chocolate," he answered. "Why"? "I don't know." An attempt was then made to test the functioning strength of the association previously established between the cutaneous stimulation and the mouth opening. Upon the first trial there was no reaction except slight general movements of the body, twitching of the hands, etc. Upon the second trial very definite mouth openings appeared but they appeared in the period before the tactual stimulation, immediately after the bandage was adjusted, as well as in the later period. This condition persisted in the third trial. In the fourth trial the reaction occurred during the cutaneous stimulation only. As the child seemed very tired the day's experiments were discontinued at this point.

After 24 hours the correct reaction occurred upon the first trial. Then an attempt was made to determine specificity or localization for the cutaneous stimulation. So far cutaneous stimulations had always been applied to a definite place, i. e., the inner side of the right arm just above the elbow. Now the same place on the left arm was also stimulated. The stimulus was applied 10″ on the left arm, then 10″ on the right arm, then the child was fed. The first time there was no reaction except a slight opening of the mouth at the moment of change from the left to the right arm, although throughout the child's body was stiff and tense.

In the succeeding trials 5″ was allowed to elapse between the stimulation on the left and right arms in order to reduce any shock of change. On the first of these trials there was no reaction except a slight stiffening and extending of the right arm before the brush was applied to it, then a very faint swallowing movement after it had been applied about five seconds. On the next or in all the third trial for specificity there was more definite reaction but only to the correct stimulation. The indications are that specificity as such, at least right-left specificity had developed with the conditioned reflex itself, although all reaction was temporarily inhibited by the introduction of the new area of stimulation.

After 24 hours specificity in reaction occurred in the first trial, then the conditions were complicated even more. The cutaneous stimulus was applied for 10″ to the left cheek, with a marked reaction. Then five seconds of no cutaneous stimulation intervened during which period the reaction persisted but gradually diminished, then the right cheek was stimulated for 10″, again with violent reaction, then 5″ intermission followed and finally came 10″ stimulation of the right arm without any reaction except very heavy breathing. In the next trial the same order of stimulation prevailed except that 10″ stimulation of the left arm was also introduced between the stimulation on the cheeks and that on the right arm. Reaction was marked to all stimulations applied except to that on the right arm to which there was no reaction. In the next trial the intermissions were omitted to shorten the series and 10″ stimulation given on each of the following places—left cheek, forehead, right cheek, left arm, right arm, without arousing reaction to any except the *new* area, i. e.,

the forehead. The child now seemed tired, said he wanted to go up stairs and see the toys, so only one more series was tried. This time the same places were stimulated as in the last trial preceding but only 2″ stimulation on each, except on the right arm where the usual 10″ was given. No reaction occurred except to the stimulation on the right arm and that developed within the first two seconds.

The next day another attempt was made to determine specificity. In the first trial there was a general reaction to all cutaneous stimulation of cheeks, forehead and both arms. This persisted in the second trial and was accompanied by marked tension of the whole body. In the third trial only the forehead and right arm were stimulated and the reaction was much more marked to the arm stimulation. On the fourth trial all areas were again stimulated but reaction was rather faint except to that of the right arm which this time was very violent.

A four days interval now elapsed while Partil was away.

On the next day of experimentation the development of specificity of reaction was again undertaken. On the first trial successive stimulation of the left arm, forehead, right cheek, left cheek, right arm for ten seconds each brought absolutely no reaction. Upon the second trial reaction occurred to all except stimulation of the left arm. Upon a third trial there was no reaction to touching the left arm, forehead and right arm and almost none to the stimulation upon the cheeks. Upon the fourth trial there was reaction only to stimulation upon the right arm and this reaction was accompanied by a smile. This discrimination was not absolutely perfected, however, but in the fifth trial reaction

occurred to the forehead stimulation also, while complete discrimination functioned in the sixth trial. This fluctuation is most interesting in connection with the following information. During the four days he was away Partil had been to visit at a place which aroused an old phobia that he had had from the age of two. Since his return he had been nervous and excitable, had not slept well and had refused to sleep in his room alone.

Two days later he seemed to be in far better condition, although he was not yet sleeping well, and came again to the laboratory. Another attempt was made at determining a specific reaction to the cutaneous stimulation. The skin was stimulated upon the left arm, left cheek, forehead, right cheek and right arm. In all seven trials were given that day. In the first trial the child was passive except for a very faint reaction to the stimulation of the left arm and left cheek. In the second trial there was a slight correct reaction to the touch upon the right arm. There was no reaction at all during the third and fourth trials, but in the fifth trial the child reacted to all stimuli, while in the sixth and seventh trials he was again absolutely quiescent.

The next day, after the first night of real rest which he had had since his trip and when for the first time he had consented to sleep in his own room, specificity was again studied. Upon the first trial all five places of stimulus application were used and all were reacted to with violent twitchings and quiverings of the body but neither here nor in any other trial made that day was Partil's mouth opened once, all reaction being violent body twitchings. In the second trial only the left arm, right cheek and right arm were touched but all gave the reaction. In the third and fourth trials the

whole five places were touched and both times the reaction was general. In the fifth trial the bandage alone was sufficient to give marked reaction which increased when the left arm was touched and continued about the same for the stimulation of the right arm. In the sixth trial there was no reaction to bandage or the touch upon the left arm or left cheek but only to the conditioning stimulation of the right arm. In the seventh trial this specificity was lacking and response occurred to the bandage itself then to left arm and forehead stimulation as well as to right arm stimulation. Work stopped at this point until the next day when four more trials were given but the results were all still non-specific.

No more attempts were made at developing specificity of the skin stimulus but instead the next day an attempt was made at the development of inhibition or unlearning of the response which had become so general. The cutaneous stimulation and bandage were applied ten seconds and the child was *not* fed. This was repeated at the same three minute interval used to develop the reaction. After 6 trials the child was quiet and on the seventh or second quiet trial he was again fed. The next trial showed a very convulsive reaction but this was as much to the bandage as to the bandage plus the cutaneous stimulus.

The question then arose of developing discrimination between these two. The bandage was at first an indifferent factor having been rendered so by habituation to it without feeding but during the series when an attempt was made to develop specificity the stimulus reaction had irradiated so as to include it. This secondary association must next be broken down and we attempted it as follows. The bandage was put on ten

seconds and no feeding was made. Then after three minutes the bandage was put on 5″, then the cutaneous stimulation was simultaneously presented for 10″ more and a feeding was made. After two repetitions of the double procedure the child lay perfectly quiet upon the third trial while in the fourth, fifth and sixth trials he reacted with increasing volume to the correct conditions only.

After 24 hours this discrimination was tested again and a rather doubtful reaction occurred to the bandage alone the first, second and third times but this was entirely lacking the fourth and fifth times, reaction occurring only in response to cutaneous stimulation.

Thirty-three days later Partil was again available and the persistence of the touch-open-mouth association tried again. The first trial there was no reaction, the second time there was a vague undifferentiating reaction to the bandage but this increased when the cutaneous stimulation was given. The reaction was still general on the third and fourth trials but occurred only in response to the cutaneous stimulation in the fourth and fifth trials.

One more series was attempted with Partil, that of the development of a memory conditioned reflex. An auditory stimulus, the ringing of a metronome, was used for ten seconds and then ten seconds after the ringing stopped the child was fed. As early as the second trial the reaction occurred, but it occurred *during* the ringing of the metronome. Gradually it came later and later after the initiation of the auditory stimulus and finally after 15 trials was delayed until the metronome has stopped.

The results of the work upon these eight children left no doubt in the mind of the experimenter as to the

feasibility of applying the method of conditioned reflexes in the study of young children. These children had been approached with the determination that their behavior itself should be made the controlling factor and should be allowed to lead to suggestions for modification of technique and method. The necessity of making the bodily position to be assumed during the study indifferent had been recognized and with active children who could not be kept lying down the whole time recourse was had to a game of lying them back on the couch (upon which they stayed even when sitting up) at irregular intervals both before the experiments began and in the intervals of the experiment. The bandage, too, could probably have been rendered indifferent in the same way but the fact, entirely unanticipated, that it proved such an important factor in the child's reactions suggested that the Russian insistence upon simplification could be carried still further in this instance and at the same time make it easier for any one person to handle the various factors of child, bandage, stimulation, feeding, apparatus and time control, while it also simplified the number of possible reactions. These could again be simplified by taking off the bandage at once after the feeding as it would eliminate the development of the child's successful attempts to remove bandage after feeding. This was done with Partil and in all later experiments.

The trial with Partil of stopping the day's work when the desired reaction was once obtained, or before if fatigue showed, proved far more commendable from the standpoint of evaluation of results as over-learning was decreased to the minimum necessary due to trials for memorial re-functioning.

That there are great individual and probably also

developmental differences seems evident, while there is no doubt left in the mind of the experimenter but that innumerable variations of the method may be applied to the solution of almost any problem of child behavior.

The development of the specificity of reaction in Partil, its fluctuation under differences in number and order of points of stimulation and in indicating neural disturbances which could not be outwardly detected and were not understood until the report of his mental disturbance was afterwards obtained, the evident fluctuations due to fatigue of a rather neuropathic organism, all indicate the exquisite sensibility of the behavior reaction here studied. The transient irradiation of inhibition from a new non-conditioning point of stimulation to the old formerly-functioning conditioned reflex is in itself a problem of high speculative interest.

Although the results are only indicative, they are positively indicative of the possibility of studying in a normal fashion hitherto untouched provinces of child development and a quantitative investigation seemed next in order.

CHAPTER VII

A QUANTITATIVE STUDY OF THE CONDITIONED REFLEX

THE group of fifty children, which was practically all of those under seven years of age living in the village and which included in three instances seven-year-old-sisters of younger children, is in some senses a selected, in others an unselected group. The village is healthfully located and simply because it is surburban lacks congestion and slums. The children are probably benefited by these better conditions. Malnutrition is not a problem in the schools and is recognized in the homes in the several cases I found, being seemingly a condition due to other causes than lack of proper feeding.

The home conditions of the children do vary and although the variation may not be as extreme as that which would be found in a large city yet visiting the homes gave data which was most enlightening. On one extreme I met the college woman, studying her children conscientiously and individually, willing and able to give me definite characterizations of each one, knowing his weak as well as strong points, interested, too, in all children, trying to establish a kindergarten for the village in the hope that it would lead to more genuine consideration of the pre-school child. Then there was the less well-trained but intelligent mother, using common sense in letting her children get daily lessons from

life through helping care for each other, encouraging Marie to dry the dishes even if one were broken, praising Jackie when he carried away the grass as father raked the lawn, too busy with three or four under school age to be theoretical but yet appealing to me in more than one instance for advice as to whether James should start school this fall or wait until he's fully six and nearly seven, making cogent observations regarding school hygiene, play and teachers. On the other extreme were those homes which seemed markedly distinguished by one peculiarity. Although the houses had at least the conventional three rooms on the first floor they were not in use as in the other homes. The front doors were closed, the front porches bare and entrance was made through the back door. The family, in other words, huddled. It ate, dressed, sewed, played, visited, in the kitchen. The members were living under slum conditions when they were absolutely unnecessary and even in the warm weather were seldom found outdoors if we except the playing children who were chased out to be out of the way. From the standpoint then of home and social environment the children were not selected.

As regards their inheritance, variation is again found. Careful observation, carefully verified, showed the following conditions. The parents of most of the children were both living and both normal, the mother running an average American middle-class home, the father working steadily, interested in his family and his home. In one instance the father was paralyzed, had been so for several years, due to a clot in the motor area of the brain. This had been detected by X-rays and was due to a sudden hemorrhage from over-exertion. He had always been rather delicate. In another

instance the mother had a goitre. In another the mother had insane spells resembling psychic epilepsy. In these spells she would strike and beat her five-year-old son without any seeming cause, then suddenly pet and nurse him to make up for it. This occurred six or eight times a day. The woman was seemingly intelligent, conversed well and is of people who are in better circumstances than she. The husband and father is a feeble-minded man with a mentality of about ten. He works steadily but cannot even be allowed to drive a team for his employer as he ruins the horses through unintentional misuse. His wages are doled out by his employer as needed else the wife would spend them all for whatever took her fancy and leave the bills unpaid. In another case the mother was rather neurotic, suffering from intense nervous headaches. In one other case the mother was that easy-going type that can manage to keep a family alive and fed due to delicatessen and bakery shops, but would, we feel, make a failure of anything where efficiency was demanded. Whether she would be classed as a social inefficient is a doubtful question. In one other case the mother was very large and obese. This condition was accompanied by a coarsened skin condition, a deep mannish voice and facial hair. The two children who have reached adolescence have at that period developed a similar great stature and obesity. The condition does not resemble a pure acromegaly but one would suspect a definite thyroid abnormality with its concomitant disturbance of other functions. The older children are above average in their school accomplishments. One other abnormality was noticed. This is probably the sort of thing that would never be detected in a survey by one not on intimate terms with some members of the community.

It is the thing that the village gossips did not talk about and this seemed surprising at first, but rather gives corroborative evidence of the fact that the condition not only exists but exists to the extent found by the investigator, escaping comment just because of its prevalence. Among the fifty children tested the history was obtained that 4 of them were the children born of "forced" marriages, while the mother of another one is reported as having been exceedingly immoral since marriage. The fact that in all but one of these instances discrepancies of the same sort are reported in other branches of the family living in other villages indicates the truth of the findings. Whether the condition is unusual or just due to a more intimate knowledge of the situation than is usually acquired cannot be determined here.

Turning now to consideration of the children themselves let us study them quantitatively.

The following mathematical evaluations of the measurements of the children studied have been made and the correlations of the most important of them calculated:

1. Chronological age, or the age of the child at the time of the experimentation, expressed in months to the last whole month, including any month completed on either of the two successive days upon which the child was studied.

2. The first process of the modified Krasnogorski experiments, or the learning ability of the child as expressed in the number of trials necessary to form a sensory-motor association. The assumption being that the fewer the number of trials necessary, the greater the learning ability.

3. The second process of the Krasnogorski experi-

ments, or the memorial functioning of the association learned after an interval of twenty-four hours, measured by the number of trials necessary to obtain its refunctioning, fewer trials meaning better memory.

4. The learning value of the retention, or the saving in number of trials necessary the second day as compared with the number necessary the first day.

5. The percentage of saving in number of trials saved from first to second day.

6. The third process of the Krasnogorski experiments, or the number of trials necessary to effect unlearning of the associative act learned when conditioning stimulus is absent. Here again the actual number of trials necessary should theoretically be fewer the greater the intelligence of the child.

7. As the number of trials needed to develop the unlearning may bear some relation to the number of trials necessary to learn the original association, the relation of these was calculated in the per cent of trials necessary for the learning that was used in bringing about the unlearning. Whether a low percentage indicates higher intelligence or not remains to be seen. If the supposition expressed in number six is true, then the same should hold in this case.

8. The fourth process of the modified Krasnogorski experiments, or the number of trials necessary to correct the unlearning developed and to restore the re-functioning of the first association. Here again the number of trials should be less, according to theory, the greater the child's intelligence.

9. The mental age of the child as expressed by his ability measured upon the Binet scale. These evaluations were not made in years but the actual number of points upon which the child succeeded was counted.

10. The mental age of the child as expressed by his ability measured upon the Bridges-Yerkes scale. Here again the evaluation was not transposed into years of mental age but left in the total number of points with which the child was credited.

11. The number of seconds the child required to complete putting all the blocks into the Seguin Form Board, taken upon the first trial upon which the child succeeded without help, three trials being given if necessary.

12. The number of seconds required to complete the Form Board in the best of the three trials given the first day.

13. Improvement upon the Form Board upon the first day, being the saving in number of seconds from the first trial to the best of the three given.

14. The percentage of the original time saved in the best trial made upon the first day. This seems to be a measure of the learning ability of the child.

15. The improvement in number of seconds saved from the best trial made on the first day to the first trial made upon the second day.

16. The improvement in number of seconds saved upon the Form Board from the best trial the first day to the best trial the second day.

17. The percentage of improvement made upon the Form Board from the best trial of the first day to the best trial of the second day.

18. Success upon the Goddard Adaptation Board as measured by the number of turns successfully completed out of the four possible successes.

19. The standing height of the child as expressed in millimeters.

20. The sitting height of the child as expressed in

millimeters.

21. The standing-sitting height index, or the standing height in millimeters divided by the sitting height in millimeters.

22. The weight of the child as expressed in kilograms.

23. The weight-height index, or the weight in kilograms divided by the standing height in centimeters.

24. The dynamometer ability of the right hand, expressed in kilograms.

25. The dynamometer ability of the left hand, expressed in kilograms.

26. The average dynamometer ability, expressed in kilograms.

27. The grip-ability—height index, being the average dynamometer ability in kilograms divided by the height in centimeters.

28. The grip-ability—weight index, being the average dynamometer ability in kilograms divided by the weight in kilograms.

29. The spirometer ability of the child as expressed in liters.

30. The spirometer-ability—height index, being the spirometer ability in liters divided by the height in centimeters.

31. The so-called vital index or the spirometer ability in liters divided by the weight in kilograms.

Any further or more detailed discussion of the significance of these various measurements will be left until we consider the significance of their relations to each other.

The interpretation of these correlations is, however, not so simple as it may seem at first. In social statistics we are usually concerned with trying to find how

far two elements, two conditions, are correlated and the mathematical expression which gives us this information is in itself understandable. In this study, also, we wish to know in how far these various measurements are correlated but that is only half of our task. In order to evaluate the correlations that we may find it is necessary to know in how far the arrays *should* be correlated, *provided* they were the expressions obtained by the use of methods of mental examination which were infallibly correct in their diagnosis of normality, defect, and genius.

If we had any such measure, indicating the various differences in mentality as correctly as the stadiometer indicates indifferences in height, the distribution of any unselected group measured by it would probably take the same characteristic form as the distribution of other anthropometric characteristics, that is, it would be a Gaussian distribution. There would be this difference, however. In-so-far as it is harder for any one to reach and surpass the ability expected of him than it is for him to remain intellectually dormant and fall farther and farther behind the constantly increasing norm, we might expect the distribution to be somewhat skewed towards the side of mental superiority.

This curve skewed towards the upper end is the typical curve that has been found in many studies of estimated intelligence through the use of marks, intelligence tests, etc. This distribution may be due to a lesser range of variation among those above normal or it may be due to our own mediocrity and consequent inability to properly discriminate and evaluate those factors which really indicate superiority. Whichever the cause this study does not claim to differ from any

of the rest and will consequently make use of the skewed form.

Goddard (59) found such a distribution in his study of over fifteen hundred school children tested by the Binet-Simon scale. Indeed, he uses the fact that the distribution is such as a proof that the scale is an accurate one. Whether this is so or not, nevertheless, it is true that the distribution is such a one as we would expect to find with a scale well-nigh perfect. This may be due to the fact that any imperfections in the scale are not general but pertain to perhaps more, perhaps fewer, of the individual tests. In the general findings these errors compensate for each other and give fairly accurate results although the individual findings may be more or less unfair. Moreover, any errors due to the functioning of the personal equation of an examiner are here probably balanced by the like errors of other examiners in the opposite direction, and so we have used the data presented by him in the table on page 234 to calculate the correlation coefficient of mental and chronological age when its expression takes the form of a Gaussian distribution. The value of this coefficient proved to be + 0.808. Using this coefficient for the sake of rough comparison we shall be able to tell more exactly than by the use of the actual correlations alone the significance of the various relations found.

The following coefficients of correlation have been calculated by the product method.

What is the significance of these findings? A separate discussion of each one is not necessary but those which seem the most important deserve further analysis.

According to chronological age the distribution of the fifty children tested is as follows:

CORRELATION COEFFICIENTS OF MEASUREMENTS MADE UPON UNSELECTED GROUP OF 50 CHILDREN

	Learning	Memorial Functioning	Memorial Saving	% of Learning for Recall	Unlearning	% of Learning for Unlearning	Redeveloped Learning	Binet	Yerkes	Best Time on Form Board	Adaptation	Average Grip	Grip-Ht. Index	Grip-Wt. Index	Lung-Cap.-Ht. Index	Vital Index
Chronological age	−0.571	−0.065	−0.495	+0.288	−0.316	−0.106	+0.081	+0.949	+0.893	−0.806	+0.708	+0.867	+0.829	+0.844	+0.827	+0.762
Learning		+0.220	+0.864	−0.508	−0.450	−0.774	+0.009	−0.588	−0.589	+0.228	−0.366	−0.376	−0.283	−0.469	−0.516	−0.548
Memorial Functioning			−0.402	+0.699	−0.306	−0.343	+0.228	−0.004	+0.014	−0.186	+0.074	−0.048	−0.042	−0.072	−0.103	−0.196
Memorial Saving				−0.890	−0.169	−0.471	−0.082	−0.538	−0.557	+0.348	−0.394	−0.335	−0.349	−0.390	−0.382	−0.388
% of Learning for Re-call					+0.026	+0.279	+0.119	+0.351	+0.383	−0.249	+0.277	+0.163	+0.202	+0.200	+0.171	+0.173
Unlearning						+0.833	+0.043	−0.251	−0.224	+0.199	−0.280	−0.335	−0.327	−0.305	−0.133	−0.066
% of Learning for Unlearning							−0.094	−0.065	+0.006	−0.069	−0.268	−0.214	−0.225	−0.163	+0.005	+0.105
Redeveloped Learning								+0.083	−0.015	−0.031	+0.097	−0.016	−0.029	−0.036	+0.063	+0.057
Binet									+0.829	−0.799	+0.716	+0.790	+0.790	+0.797	+0.769	+0.739
Yerkes										−0.798	+0.672	+0.739	+0.744	+0.755	+0.722	+0.679
Best time on Form Board											−0.603	−0.596	−0.632	−0.643	−0.468	−0.356
Adaptation												+0.620	+0.586	+0.432	+0.519	+0.459
Average Grip													+0.989	+0.952	+0.867	+0.678
Grip-Ht. Index														+0.960	+0.791	+0.682
Grip-Wt. Index															+0.774	+0.889
Lung Cap.-Ht. Index																+0.968

Age in mos.	*Boys*	*Girls*	*Totals*
12-23	2	3	5
24-35	5	2	7
36-47	3	3	6
48-59	4	8	12
60-71	4	4	8
72-83	6	3	9
84-89	0	3	3
Totals	24	26	50

The predominance of girls is to be regretted but is due simply to the sex ratio in the village studied.

First, the number of trials necessary for the formation of an association between stimulus and mouth opening for food does not vary sufficiently with the different chronological ages to have a sufficient value for indicating with a high degree of accuracy the differences of learning ability at the different chronological ages, *if* such differences exist. The correlation coefficient or r of the number of trials with chronological age is only — 0.571 while in our criterion of a sufficiently high value in a Gaussian distribution r is plus or minus about 0.80.

Let us turn, however, to the original data and inspect it further. We find that the number of trials required by the various children ranges from nine to three. As two correct trials were required before the association was counted as learned, and as one additional trial, the initial one, was needed to introduce the situation and replace a verbal Aufgabe, three is the smallest possible number of trials in which a child could develop the correct functioning of the association. Krasnogorski uses the first correct response as indicating learning,

however, and so the records of all these cases, according to his estimation, would lie within the range of two to eight trials. Krasnogorski (100) reports that two to ten trials are sufficient. The difference in the upper limit of the range may be due to the fact that he used younger children than those twelve months old who were the youngest used by the experimenter in the present study, or to time differences, or may be those variations due to differences in the strength of stimuli, method of presentation, or, as is quite likely, to the differences between children being handled by a man and by a woman.

If we analyze the data for the number of trials required by the children of different chronological ages, using whole years because of the small number of cases, the following results are obtained:

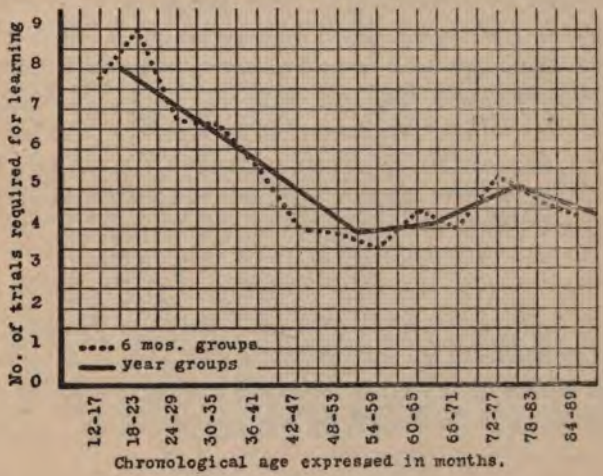
No. of Cases	*Age*	*No. of Trials (Av.)*	*Range*	*Mean Var.*
5	12-23	8.0	7-9	0.8
7	24-35	6.71	6-8	0.82
6	36-47	5.33	3-8	1.44
12	48-59	3.83	3-5	0.83
8	60-71	4.12	3-5	0.44
9	72-83	5.0	3-7	1.11
3	84-89	4.3	4-5	0.44

These results may be seen expressed in curves on Chart I.

It is clearly to be seen that up until the age of five the number of trials necessary for the formation of the conditioned association decreases rather regularly as the chronological age of the child increases. Above this the curve is not only less regular but the range of trials necessary for any one age is also greater than

for the ages just preceding. This can hardly be due to any great difference in the ability of the older children as a group, for they were in most instances (in 15 out of 20) the older brothers and sisters of the younger children used. It may be possible that we have here a symptom of an innate difference of different periods of development. The older child may see or imagine,

CHART 1. RELATION OF LEARNING TO CHRONOLOGICAL AGE.



because of his greater experience, and consequent greater potentiality of associations, possibilities of variation in the procedure to which the younger child is oblivious, being absolutely sure after he has been fed candy under a given condition once or twice that it will appear again under like conditions. Genetically viewed, this difference may be as significant as a mark of old stages of development as are the differences recognized to-day between the adolescent and the pre-adolescent.

Calculating the correlation coefficient for the num-

ber of trials necessary for the formation of the association with chronological age for the children under 60 months of age we obtain r. equals — 0.816. As the one is a decreasing array and the other an increasing one, the relation is a positive one and comparison with the coefficient of the high value Gaussian distribution assumed as a tentative norm, 0.808, indicates that it has undoubted value as a basis for distribution.*

It may be interesting to note that no child over two years of age needed more than eight trials while none under that age used less than seven, none under three years needed less than six, while the minimum number, three, was all that were required by a child in the fourth year. Out of the fifty children, regardless of age, ten needed only three trials, eleven needed four trials, eleven used five trials while only seven needed six, five needed seven, four needed eight and two, nine trials.

If we analyze this data still further and consider the sexes separately we see that there is in sex yet another factor determining differences in the rate of learning. This shows in the table following although some of the differences are obscured by the age grouping.

No. of Cases	*Age in Months*	*Av. No. of Trials for Learning*	*Range*	*Mean Var.*
Boys 2	12-23	7.0	7	0.00
Girls 3		8.7	8-9	0.43
Boys 5	24-35	6.4	6-8	0.64
Girls 2		7.5	7-8	0.50
Boys 3	36-47	6.33	4-8	1.59
Girls 3		4.33	3-5	0.89

* Some such criterion of the correct value of r must be assumed when diagnostic norms are being studied. An r too near ± 1 means that the factors under observation correlate too highly for the one to be used in selecting variants from a group which will not vary

No. of Cases	*Age in Months*	*Av. No. of Trials for Learning*	*Range*	*Mean Var.*
Boys 4	48-59	4.50	3-5	0.75
Girls 8		3.50	3-5	0.62
Boys 4	60-71	4.50	4-5	0.50
Girls 4		3.75	3-4	0.38
Boys 6	72-83	4.67	3-6	1.11
Girls 3		5.67	4-7	1.11
Boys 0	84-89			
Girls 3		4.33	4-5	0.44

Expressed as a curve for the purpose of easier comparison, this table gives the distribution seen on Chart II.

Pursuing the analysis we find that almost without exception the average age of the boys decreases as the number of trials necessary for learning increases. With the girls, however, the age-learning relation is much less definite. The mode for learning with the boys is at five trials while for the girls it is at four, although the number there is almost equaled by the number learning in three trials. This may be seen in the following table:

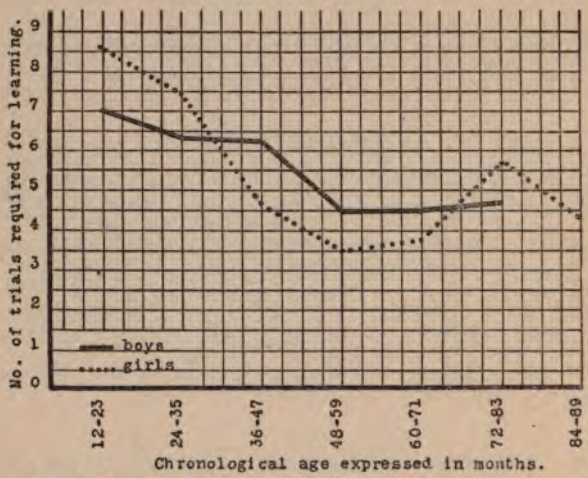
RELATION OF LEARNING TO SEX AND AGE

No. of trials needed for learning.....	3	4	5	6	7	8	9	Total
No. of boys.....	3	3	7	6	3	2	..	24
Average age....	69	62.7	62.3	44.5	20.6	34.5		
No. of girls.....	7	8	4	1	2	2	2	26
Average age....	52.6	69.1	53.8	77.0	52.5	21.5	18.0	

in the other factor also. For instance, a mental test which has an r of $\pm$ 0.95, or so, with chronological age is apt to indicate an ability so dependent upon chronological age that it varies with chronological age independent of actual mental ability.

The correlation between chronological age and the number of trials needed for memorial functioning of the association after 24 hours is — 0.065, which shows such a slight decrease as the age increases as to appear negligible. Again recourse to our actual figures may be helpful. Since two trials were required before the association was counted as functioning that is the smallest number that could be used. Thirty-six of the fifty children gave a positive reaction on the first and

CHART II. RELATION OF LEARNING TO CHRON. AGE AND SEX.



second trials, 9 on the second and third trials, while one took 4 trials, two required five, one needed six and one seven trials. Evidently in most of them the association was so firmly developed that a twenty-four hour interval had not reduced it sufficiently to bring it below the functioning limen. Nor is the distribution of those who needed more than two or three trials greatly influenced by age. The one who required seven trials was 12 months old but the one who required 6 trials

was 77 months old and the distribution of the rest was very irregular.

The correlation of the number of trials needed in learning with the number needed after the 24-hour interval is + 0.22, or a slight indication that those who needed more trials for learning were not, as is usually supposed, more retentive but required likewise a greater number of trials to re-establish the association. Some interesting relations of the learning and memorial function are seen in the following table:

RELATION OF MEMORY TO LEARNING

No. of Trials for Learning	*No. of Cases*		*Range of Trials for Remembering*	*Av. No. of Trials for Remembering*	*Mean Deviation*
3	Boys	3	2	2.00	.00
	Girls	7	2	2.00	.00
	Both	10	2	2.00	.00
4	Boys	3	2	2.00	.00
	Girls	8	2-5	2.50	.75
	Both	11	2-5	2.36	.59
5	Boys	7	2-6	3.29	1.04
	Girls	4	2	2.00	.00
	Both	11	2-6	2.82	.89
6	Boys	6	2-3	2.33	.43
	Girls	1	2	2.00	.00
	Both	7	2-3	2.29	.41
7	Boys	3	2-7	3.70	2.23
	Girls	2	3-5	4.00	1.00
	Both	5	2-7	3.80	1.76
8	Boys	2	2-3	2.50	.50
	Girls	2	2-3	2.50	.50
	Both	4	2-3	2.50	.50
9	Girls	2	2	2.00	.00

These results do not confirm the rather generally accepted thesis that the greater the number of trials necessary for learning, the more efficient the reproduction, as both the most facile and the slowest learners are more efficient than those intermediate in type.

Let us turn now to the third Krasnogorski process, or the unlearning of the previously developed association. It was found impossible to effect this unlearning without severe emotional disturbances in the youngest nine of the children tested. The palliative effect of the candy in the learning experiment was more vividly understood when this attempt was made to use the bandage and not feed. Usually the second, always the third or fourth trial, brought crying and no further attempt was then made, chiefly because of the unfavorable effect of the crying upon the other children playing near, and the effect of reported crying upon the mothers of the community. No difficulty was experienced with children over twenty-eight months of age. The group in whom unlearning was developed consequently includes only 41 children ranging in age from 29 to 89 months.

The average rate of unlearning for children of the different ages and sexes may be seen in the table on page 155.

From this we can see that there is in general a decrease in the number of trials needed for unlearning as the chronological age increases. The average number of trials needed by boys and girls of the same chronological age likewise differs considerably and there is an accompanying difference in the range of number of trials and in variability in the two sexes.

The correlation between the number of trials necessary to effect the unlearning and chronological age is

No. of Cases	*Age in Months*	*Av. No. of Trials for Unlearning*	*Range*	*Mean Var.*
Boys 4 Girls 5	12-29	Failure Failure		
Boys 3 Girls 0	30-35	6.33	5-8	1.11
Boys 3 Girls 3	36-47	8.33 9.67	5-12 9-10	2.44 0.44
Boys 4 Girls 8	48-59	7.00 8.38	3-9 5-11	2.00 1.63
Boys 4 Girls 4	60-71	6.50 9.50	5-9 7-12	1.50 2.00
Boys 6 Girls 3	72-83	6.33 5.33	4-10 4-7	1.78 1.11
Boys 0 Girls 3	84-89	5.33	5-6	0.44

— 0.316, which corroborates the fact shown by the table above that there is only a slight decrease in the number of trials needed as age increases. The number of trials necessary to unlearn the association varies more than the number required in the learning of it. The distribution frequency is as follows:

No. of trials required.	3	4	5	6	7	8	9	10	11	12	Failures
No. of cases.	1	2	10	4	4	6	5	5	2	2	9

This table indicates a double distribution in the rate of developing unlearning, the one with a mode of 5 trials, in some instances approaching in ability the

minimal limit of the task, the other type more variable among themselves with a mode less well expressed but lying probably at 8, 9 or 10 trials.

The extremes of number of trials necessary to effect unlearning are those mentioned by Krasnogorski for although he makes the general statement that it develops in 5 to 10 trials in normal children he also says in one instance that the association broke down after three trials in one six-year-old while in another instance it took eleven trials without stimulation before on the twelfth the reaction was lacking. But as there may have been great differences in method, length of stimulation times and intervals in the work he reports and that reported here this coincidence may not be assumed to mean corroboration of the accuracy of the findings in either of our series. If he used only one trial without positive response as an indication of the degeneracy or decreased functional efficiency of the conditioned reflex then by comparison we again, as in the learning process, find the mechanism more easily developed in our series.

If we analyze the results on unlearning in their relation to the number of trials needed in the original learning we find r equals — 0.450, while in the relation between unlearning as measured in per cent of the learning trials it required and learning itself r equals — 0.774. Tabularly distributed this relation may be seen on page 157.

In general the greater the number of trials that was necessary to develop the conditioned reflex the less the number required to break it down.

If we analyze these results still further, according to sex, we find the distribution for either sex is quite different from that of the other.

RELATION OF LEARNING TO UNLEARNING

No. of Cases	*No. of Trials needed to effect Unlearning*	*No. of Trials used in Learning*						*Av. No*
		3	4	5	6	7	8	
1	3			1				5
2	4				1	1		6.5
10	5		2	4	3		1	5.4
4	6	1	1	1	1			4.5
4	7		3	1				4.25
6	8	2	3				1	4.3
5	9	3	1	1				3.6
5	10	2		3				4.2
2	11	1	1					3.5
2	12	1				1		5.0
Average No.		9.2	7.4	5	5	8	6.5	

SEX AND AGE DISTRIBUTION WITH NUMBER OF TRIALS FOR UNLEARNING

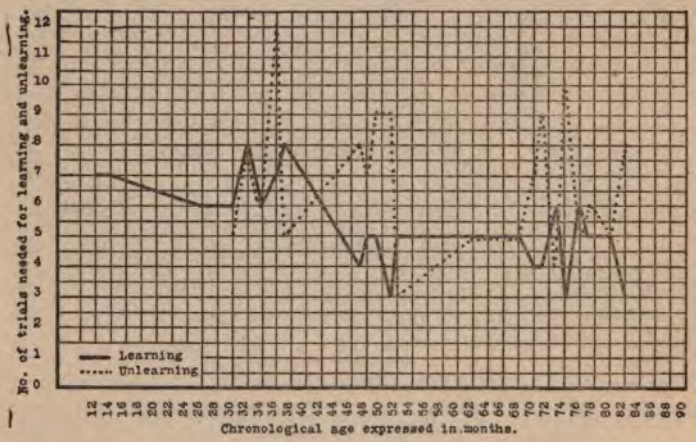
No. of trials for Unlearning	3	4	5	6	7	8	9	10	11	12	F.*
No. of Boys	1	1	6	2	2	3	3	1	0	1	4
No. of Girls		1	4	2	2	3	2	4	2	1	5
Av. No. of trials in Learning											
Boys	5	6	5.8	5.5	4.5	5	4	3	...	7	6.5
Girls		7	4.8	3.5	4	3.7	4.35	4.5	3.5	3	8
Average age, Boys	52	73	58.8	55.5	59	50.3	57	74	...	36	20
Girls		80	75.8	71.5	70.5	59	45	44.5	55.5	70	20.8

* F = Failures.

The range of distribution of the boys is wider, according to the number of trials used, than is that of the girls although the opposite was true of the learning process. The most frequent form of development of the unlearning took five trials with the boys while with the girls the development in five trials and development in ten trials occurred with equal frequency. With the boys the learning rate for those who needed five trials or less for unlearning was slower than it was for any

of those who needed more trials if we make an exception of one case who took twelve trials to unlearn and had needed seven to learn. With the girls the number of trials for learning is highest for those needing four or five trials to effect the unlearning and then gradually decreases as the difficulty of unlearning increases but with another rise in learning difficulty for those who had taken ten trials to unlearn the association.

CHART IIIa. RELATION OF LEARNING AND UNLEARNING TO CHRON. AGE AND SEX (BOYS).

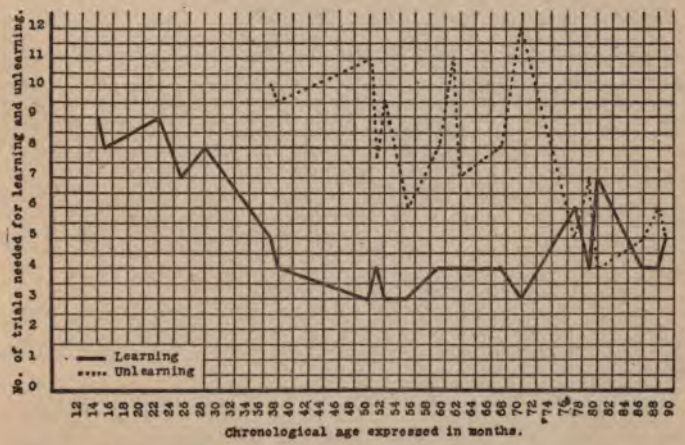


With the boys we have an almost homogeneous group, finding less difficulty in developing unlearning the more slowly they learned or the less plastic they were to receive the more mobile they were to erase or modify. With the girls we have one group learning slowly and unlearning easily and also a group learning slowly and unlearning with difficulty, while those who learn more readily unlearn with proportionately increasing difficulty. Have we here in these early differences an indication of conditions which later lead to the much more

frequent nerve disturbances among girls? The fact already pointed out by Krasnogorski that neuropathic cases are apt to unlearn conditioned reflexes very slowly seems to re-enforce the probability of any such an inference.

Although less accurate than a rough polygon would be, Charts III a and III b show more clearly the relations age, sex, learning and unlearning hold to each

CHART IIIb. RELATION OF LEARNING AND UNLEARNING TO CHRON. AGE AND SEX (GIRLS).



other. Above 6 years, i. e., 72 months of age, our data is too scanty to enable us to analyze what seems to be a very complex matter but below that the differences are more clearly seen.

Up until the age of two years and a little over the girls learn more slowly than the boys. From the age of about two until six they learn more rapidly than boys of the same age with only one exception.

Unlearning, on the other hand, is in every case but one harder for the girls than for boys of the same age

up until six years of age.

The fourth mechanism studied was that of re-functioning of the conditioned reflex which had been unlearned. Krasnogorski uses the term "breaking-down" or "degenerescence" to denote the process which I have called unlearning. Those two terms together with a consideration of this fourth process itself give a better insight into its mechanism. In all of the 41 cases in whom the destructive effect of stimulating without feeding was seen the unlearning was neutralized and learning redeveloped with little difficulty. Two successive reactions were necessary to be sure the response was not accidental and in 38 of the children the first two after the renewed feeding were all that were necessary as they reacted positively from the first renewed feeding. In two cases three trials were necessary and in one case four. It is interesting to note that the two cases needing three trials were both over six years of age while the one needing four trials was only 51 months old, a child rather above usual intelligence, who formed a conditioned reflex in five trials but who needed ten trials to effect unlearning.

As might be expected from this uniformity of results correlation of redevelopment of learning with other functions is almost lacking, r for chronological age being only $+ 0.081$ and the highest, that with the number of trials needed for memorial functioning of the conditioned reflex, being only $+ 0.228$ and consequently insignificant.

The regularity of these results leads to a different view concerning this fourth process. It is not the rebuilding of an association that has been broken down completely although reaction is completely lacking. Instead the mechanism of unlearning may be looked

upon as a process which reduces the strength of the association just far enough to take it below the limen of response and consequently only enough new associations are needed to raise it once more above the threshold. It seems relatively valueless as a method of studying mental processes when used in this form. Undoubtedly a study of the functioning of the redeveloped association, comparing it with its primary functioning before modification by unlearning, would throw still more interesting light upon the relation of the learning and unlearning processes. At least the field is one of speculative interest although the results I have obtained throw very little light upon the mental processes of the child in this respect.

Before making any further analysis of these processes it will be well to study the group from the standpoint of other tests given.

RESULTS OBTAINED BY OTHER TESTS

In evaluating the measurements obtained by the use of the Binet and Bridges-Yerkes scales all calculations have been made upon the total number of points gained. This gives one a chance to study the value of the test series apart from any arbitrarily determined age value which has been assigned. This seemed especially important for the Binet evaluations as most people fail to recognize in them a point scale from which the Yerkes scale differs chiefly in that it has selected a limited number of questions, chiefly from the Binet, and has then made a higher number of points of gradation possible through more minute qualitative classification and a consequent higher evaluation of results than they were given in the Binet.

The correlation between chronological age and Binet

grading is + 0.949. This is much higher than the indicated correlation of a Gaussian distribution. Whether this is an error or not is difficult to decide. The tests surely do not pick out in a prognostic manner any large number of deviates from the norm. Whether there are, however, significant deviations present at this early age in a larger proportion of children is still an unanswered question.

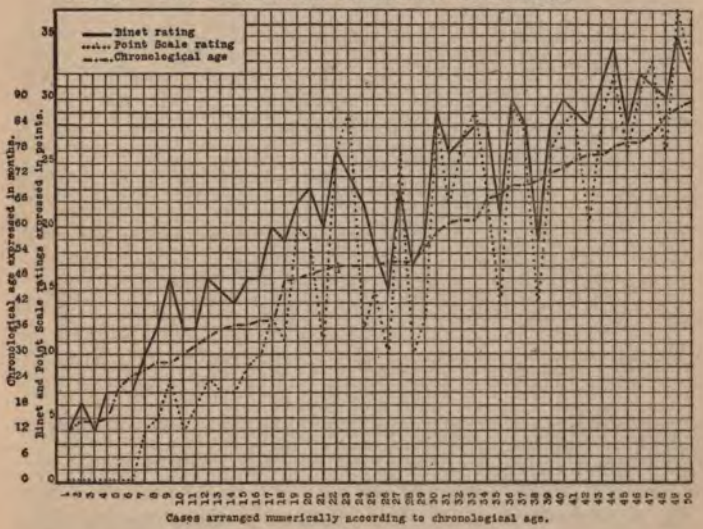
The correlation of the Yerkes evaluations and age is + 0.893. This is probably a more highly prognostic distribution. The inventor of this scale might justly criticize the usage of it on the younger children since he states (191, p. 89) that the scale is of little value below the age of three years and seven months. As a matter of fact it gives a rather regular distribution, approximating that of the Binet, between the ages of two and four, and above that age it differs little from the Binet in general trend but because of higher evaluation of each test it gives greater individual variations. This may readily be seen in the following curves which show the relative achievement of each child on the two scales, considered with respect also to chronological age.

An analysis of the ratings by the Binet or Yerkes tests based upon sex brings out very little that is new. The individual cases of either sex are now higher, now lower, in the number of points they receive credit for although in general children of about the same chronological age seem comparable regardless of this factor. But with such a small number of children, all in the period where a few months' development may mean great differences in the ability to do tests, there is no adequate basis for a study of sex differences. The number of children of both sexes who are of any one

chronological age is too small and too irregularly divided between the two sexes to make it worth while presenting the comparisons in detail.

If we analyze out the various cases falling below the general level by either, or rather by both, Binet and Yerkes tests, since in all the grosser instances these

CHART IV. RELATION OF BINET AND POINT SCALE RATINGS TO CHRONOLOGICAL AGE.


Binet rating
Point Scale rating
Chronological age
Binet and Point Scale ratings as compared with Chronological age
Cases arranged numerically according to chronological age.

coincide, we find the following to be the children who deviate the most (See Chart IV).

Case 26, female, is the daughter of the woman described as immoral since married. The child to a clinician appears dull in many ways, needs to be told things again and again and is very babyish. She is undoubtedly the dullest girl studied.

Case 38 is the son of the feeble-minded man and the insane woman.

Case 29 is the daughter of the woman with goitre

and seems to be rather slow about doing things but does not seem to be really defective.

Cases 28 and 35 are brothers. They are both called dull when compared with their older sister, case 50, but although below their compeers are not feeble-minded so far as can be determined. They are of normal but rather inferior, slow, easy-going stock.

The relation of the evaluations by either of these methods to the Krasnogorskian results is not so simple.

If we study the mental age of the group when distributed according to the number of trials they used for learning the conditioned association, regarding also age and sex, we find evidences that the relation is very complex. On the Binet all the averages for boys except for those taking 8 trials for learning show that the Binet age increases as the chronological age increases for each of the sub-divisions according to the number of trials needed for learning and that in any group of the same age the mental age decreases as the number of trials needed for learning increases. In the two cases taking 8 trials for learning the mental age is slightly greater than for those taking seven trials or less but this variation is probably due to chance working upon such a small number of cases. Also, the younger boys tend towards slower learning.

Exactly the same condition holds for the mental age of boys calculated by the Yerkes scale except that the differences according to chronological age and rate of learning seem accompanied by greater but less regular variations in mental age.

The distribution of the girls on both scales seems far less regular. Under any one of the groups formed by division according to the number of trials used on the learning the mental age increases with the chronological

age, except in the group requiring four trials which includes at the points marked ° the two brightest children for their ages that were tested, while ° ° indicates case 26, the dullest girl tested.

The relation, under any one chronological grouping, of mental age and the number of trials required to form the association is not regular although it indicates that in general the number of trials necessary is less with some dull children than with average children while other dull children take longer to learn than the average child. Only children under three years of age take as many as eight or nine trials and here the duller child of an equal age takes longer to learn.

These relations may be best seen in the tables following. The correlation between learning and Binet rating is — 0.588 while with evaluations by the Yerkes scale it is — 0.589. In the tables the figures represent the

RELATION OF LEARNING TO BINET WITH ANALYSIS BY SEX AND AGE

Age in months		*Number of trials required to develop learning.*						
		3	4	5	6	7	8	9
12–23	Boys					5		
	Girls						7	4
24–35	Boys				12.5		12	
	Girls					7	16	7
36–47	Boys		19					
	Girls	16		17		15	16	
48–59	Boys	24		20.6				
	Girls	19°°	27.5°	22				
60–71	Boys		24.5	24				
	Girls	28	27.3					
72–83	Boys	30.5		29.5	28.5			
	Girls		34°		28	32		
84–89	Girls		32.5	32				
Av. Age by Binet	Boys	28.3	22.7	24.1	17.8	8.3	14.0	...
	Girls	19.9	29.5	22.0	28.0	19.5	11.5	5.5
	Both	22.4	27.6	23.4	19.3	12.8	12.8	5.5

RELATION OF LEARNING TO YERKES WITH ANALYSIS BY SEX AND AGE

Age in months		*Number of trials required to develop learning.*						
		3	4	5	6	7	8	9
12–23	Boys					0		
	Girls						0	0
24–35	Boys				5.25		6	
	Girls					0	8	
36–47	Boys		11			7	9	
	Girls	10		10				
48–59	Boys	29		16.3				
	Girls	15°°	27.5°	12				
60–71	Boys		22	20.5				
	Girls	28	24.3					
72–83	Boys	30.5		27.5	27.5			
	Girls		32°		20	31		
84–89	Girls		31.5	33				
Av. Age by Yerkes	Boys	30.0	18.3	20.6	12.7	2.3	7.5	
	Girls	16.1	28.0	16.2	20.0	15.5	4.0	0
	Both	20.3	25.4	19.1	13.7	7.6	5.8	0

average rating by Binet or Yerkes tests of the children who by sex, age and number of trials needed for learning belong in a group. The ratings are all expressed in points for which credit was given.

If we make the same type of distributive analysis upon the basis of the number of trials necessary to develop unlearning we find that for both the boys and the girls the mental age increases as the chronological age within each and every subdivision made according to difficulty of unlearning. This is true on both the Binet and Yerkes distributions but with one exception in both. In the group needing 9 trials for unlearning there is a deviation, the case marked #, which is case 38, the son of the feeble-minded man and the insane woman.

If within any one division according to chronological age we compare the mental ages of those requiring

different numbers of trials to unlearn we find sex differences. The mental ages of the boys increase as the number of trials increases up to a certain point, then decrease as unlearning becomes still more difficult.

RELATION OF BINET AGE TO UNLEARNING, AGE AND SEX.

Age in months		*Number of trials required to develop unlearning.* 3	4	5	6	7	8	9	10	11	12	*Failures.*
12 to 23	Boys	..	..	..	..	..	..	..	..	..	..	5.0
.....	Girls	..	..	..	..	..	..	..	..	..	..	5.6
24 to 35	Boys	..	..	12.0	16.0	..	12.0	..	..	..	..	11.0
.....	Girls	..	..	..	..	..	..	..	..	..	..	11.5
36 to 47	Boys	..	..	16.0	..	..	19.0	..	..	..	15.0	..
.....	Girls	..	..	..	..	..	..	16.0	17.0	..	..	..
48 to 59	Boys	17.###	..	..	..	22.0	..	23.5	..	..	..	..
.....	Girls	..	..	26.0	19.°°	..	23.5	23.0	18.5°	20.0	..	..
60 to 71	Boys	..	..	24.0	..	30.0	..	19.#	..	..	..	..
.....	Girls	..	..	..	..	28.0	28.0	..	..	26.0	28.0	..
72 to 83	Boys	..	28.0	28.5	31.0	..	31.0	..	30.##	..	..	..
.....	Girls	..	32.0	28.0	..	34.0	..	..	..	..	..	..
84 to 89	Girls	..	..	31.0	35.1	..	..	..	..	..	..	..
Av age by Binet	Boys	17.0	28.0	22.2	23.5	26.0	20.7	22.0	30.0	..	15.0	8.0
	Girls	..	32.0	29.0	27.0	31.0	25.0	19.5	17.8	23.0	27.0	8.2
	Both		20.0	24.9	25.2	28.5	22.8	21.0	20.2		21.5	8.1

RELATION OF BRIDGES-YERKES AGE TO UNLEARNING, AGE AND SEX.

Age in months		*Number of trials required to develop unlearning.* 3	4	5	6	7	8	9	10	11	12	*Failures.*
12 to 23	Boys	..	..	..	..	..	..	..	..	..	..	0.0
.....	Girls	..	..	..	..	..	..	..	..	..	..	0.0
24 to 35	Boys	..	..	4.0	8.0	..	6.0	..	..	..	..	4.5
.....	Girls	..	..	..	..	..	..	..	..	..	..	4.0
36 to 47	Boys	..	..	9.0	..	..	11.0	..	..	..	7.0	..
.....	Girls	..	..	..	..	..	..	10.0	10.0	..	..	..
48 to 59	Boys	10.###	..	..	8.0	20.0	..	24.0	..	..	..	..
.....	Girls	..	..	26.0	13.°°	..	22.0	26.0	11.°°	11.0	..	..
60 to 71	Boys	..	..	20.5	..	30.0	..	14.#	..	..	..	..
.....	Girls	..	..	..	..	29.0	23.0	..	..	22.0	28.0	..
72 to 83	Boys	..	26.0	27.5	29.0	..	33.0	..	28.##	..	..	..
.....	Girls	..	31.0	20.0	..	32.0	..	..	..	..	..	..
84 to 89	Girls	..	..	29.5	37.0	..	..	..	..	..	..	..
Av. age by Yerkes	Boys	10.0	26.0	18.2	15.0	25.0	16.7	20.7	28.0	..	7.0	2.2
	Girls	..	31.0	26.2	25.0	30.5	22.3	18.0	10.5	16.5	28.0	1.6
	Both		28.5	21.4	19.0	27.8	19.5	19.6	14.0		17.5	1.9

This seems to be the situation although a complete distribution at any one age is lacking but the cases of lower mentality are on the ends of the various distributions according to number of trials needed. Case # is the case 38 mentioned above. Case ### is case 28 also mentioned before as rather dull, while case ## indicates the child from the family where there

seems to be some glandular disturbance which brings abnormal obesity from adolescence. This boy as yet shows no physical signs of the abnormal development.

Among the girls the duller ones seem intermediate in range while the brighter ones unlearn either more rapidly or more slowly. Case ° is number 26 described above while case °° is the daughter of the woman with goitre.

In general it seems evident that rating the child by either Binet or Yerkes and at the same time placing him relatively on the Krasnogorski processes brings out complementary evidence, from the two extremely different measures, of the child's ability relative to the group.

Let us consider next the results obtained upon the Seguin Form Board. In all only 39 children were able to complete the task of placing each of the ten blocks where it belonged. Of the eleven who failed nine failed completely and the other two were unable when they had incorrectly covered a hole to go ahead and correct their error. All children were given three trials one right after the other and no more than three. For the 39 who succeeded the following correlation with chronological age were calculated:

Initial time	— 0.774
Best time first day	— 0.806
Improvement first day	+ 0.011
% improvement first day	+ 0.442
Improvement from best first day to first trial second day	— 0.302
Improvement from best first day to best second day	— 0.482
Improvement from best first to best second day in %	— 0.512

The initial time on the Form Board is not a valuable nor a really correct value with several of the children as it was necessary to help them in order to enable them to comprehend what was wanted from them. None of the other methods of rating their ability has a sufficiently high correlation coefficient to be worth studying intensively except the best Form Board time the first day. This was the rating which was consequently selected for correlation with other findings on the same children as well as for analysis independently.

The *r* between Form Board time on the best trial and Binet age is — 0.799, or the time decreases as the mental age increases. The correlation with the Yerkes age is very nearly the same, being — 0.798. The correlations between Form Board time and the development of learning and unlearning are very low, indeed negligible, being only + 0.228 and + 0.199 respectively. A tabular analysis gives very little more information regarding the reason for this non-conformity. A curve plotted on the individual cases arranged in order of merit or speed on the Form Board shows that the rate of completing the task decreases as age or as mental age decreases. There is some relation to learning: those who learn most quickly are not, however, the quickest on this task but rather nearer the middle of the group, while the slowest learners are slowest here also. The girls seem somewhat more rapid than the boys.

The lack of a higher correlation may probably be explained in the following manner. A child who forms a conditioned reflex or conditioned association rapidly *may* learn just as rapidly on the Form Board *but* in his learning if there be any error due to a false move or to trial and error learning in general he learns that false move as a part of the procedure and he then has

to unlearn it before it drops out of his behavior. This lengthens his total reaction time and may persist for a number of trials. Such an explanation can not be proven to be correct without micromotion study but experience leads the writer to believe that it may be at least one of the factors at work.

The accompanying tables give the results obtained from an analysis of the Form Board times in the groups homogeneous as regards age, sex, learning and unlearning ability. The figures in the tables indicate average times for the groups expressed in seconds.

RELATION OF FORM BOARD TIME TO LEARNING, AGE AND SEX.

		Number of trials needed for learning.						
Age in months		3	4	5	6	7	8	9
12 to 23	Boys.....	..	..	..	..	2F.	..	..
.....	Girls.....	..	..	..	..	..	F.	2F.
24 to 35	Boys.....	..	..	..	107+2F.	95.0	145.0	..
.....	Girls.....	..	..	..	..	F.	F.	..
36 to 47	Boys.....	..	90.0	..	F.	..	99.0	..
.....	Girls.....	127.0	..	60+1F.	..	..	..	..
48 to 59	Boys.....	52.0	..	75.7#	..	..	..	..
.....	Girls.....	65.9	33.2	73.0	..	..	..	..
60 to 71	Boys.....	..	42.0	33.0	..	..	..	..
.....	Girls.....	27.0	31.3	..	..	..	..	..
72 to 83	Boys.....	31.7	..	24.5	28.0	..	..	..
.....	Girls.....	..	21.0	..	28.0	23.4	..	..
84 to 89	Girls.....	..	22.0	22.0	..	..	..	..

F = Failure.

Average time	Boys.....	41.5	58.0	51.5	54.3	95.0	122.0	..
	Girls.....	69.1	28.2	51.7	28.0	23.4	..	..
	Both.....	60.9	36.3	51.6	47.0	59.2	122.0	..

RELATION OF FORM BOARD TIME TO UNLEARNING, AGE AND SEX.

		Number of trials to develop unlearning.										*Fail-*
Age in months		3	4	5	6	7	8	9	10	11	12	*ures*
12 to 23	Boys	..	..	..	..	..	..	..	..	..	..	2 F.
.....	Girls	..	..	..	..	..	..	..	..	..	..	3 F.
24 to 35	Boys	..	..	107.0	..	..	145.0	..	..	..	..	2 F.
.....	Girls	..	..	..	..	..	..	..	..	..	..	2 F
36 to 47	Boys	..	..	99.0	F.	..	90.0	..	..	..	95.0	..
.....	Girls	..	..	..	..	..	..	127.0	60+1F.	..	..	..
48 to 59	Boys	135.0	..	..	..	33.0	..	20.5	..	..	..	..
.....	Girls	..	..	32.4	38.0	..	67.5	49.4	55.5	123.0	..	..
60 to 71	Boys	..	..	33.0	..	30.0	..	54.0	..	..	..	..
.....	Girls	..	..	..	..	27.0	34.0	..	..	33.0	27.0	..
72 to 83	Boys	..	28.0	24.5	28.0	..	24.0	..	39.4	..	..	..
.....	Girls	..	23.4	28.0	..	21.0	..	..	..	..	..	..
84 to 89	Girls	..	..	21.0	24.0	..	..	..	..	..	..	..

F. = Failure.

Average Time	Boys	135	28.0	53.5	28.0	31.5	86.3	58.3	39.4	..	95.0	F.
	Girls	..	23.4	25.6	31.0	24.0	49.7	88.2	57.0	78.0	27.0	F.
	Both		25.7	42.3	30.0	27.8	68.0	70.3	52.6		61.0	F.

In studying the relation of Form Board times to the rate of unlearning of an association we can see that in general the rapid unlearners succeed more rapidly with this task but the deviations are marked in both directions

The results obtained upon the Adaptation Board are interesting in spite of the small number of cases. They extend downward the results published by Goddard. No child under thirty months of age succeeded in correctly responding to any one of the trials. No child under 47 months of age completed the whole four turns while no child over 77 months failed. In this intermediary group success upon the first two trials is by no means synonymous, as Goddard seems to think, with success upon the third and fourth trials although it may be in older children. Of the 33 between the ages of 47 and 77 months 3 did only 1 turn, 8 did 2 turns, 3 did 3 turns, 10 did 4 turns and 9 failed completely. The correlation with chronological age is + 0.703, which is surprisingly high when one considers the few possible types of behavior in response to the situation. The correlation with Binet age is + 0.716 and that with the Yerkes is + 0.672 with a slight superiority of the Binet evidenced as a means for studying adaptability. This slight difference of the two is also seen in a slightly higher correlation of the Yerkes with the reverse function, unadaptative memory, as measured by the Krasnogorski process of unlearning.

The correlation value of the adaptation board ability with learning is — 0.366 and with unlearning is — 0.280. The difference here, although slight, again corroborates the fact that the learning and the unlearning processes do involve adaptation as a factor making for success. Disregarding age, which we have

seen is highly correlated with success on this test, we find the relation of learning and unlearning to the number of successful turns on the Adaptation Board clearly shown in the following tables:

RELATION OF LEARNING TO ADAPTATION.

Number of trials needed for learning.

		3	4	5	6	7	8	9
Boys	Failed	2	.	2	2	2	2	..
	Succeeded on 1 turn	..	..	..	..	..	..	..
	2 turns	..	..	2	1	1	..	..
	3 turns	..	1	1	1	..	..	..
	4 turns	1	2	2	2	..	..	..
Girls	Failed	2	..	1	..	1	2	2
	Succeeded on 1 turn	2	1	..	..	..	..	..
	2 turns	1	1	2	1	..	..	..
	3 turns	..	..	..	..	..	..	..
	4 turns	2	6	1	..	1	..	..

RELATION OF UNLEARNING TO ADAPTATION.

Number of trials needed for unlearning.

		3	4	5	6	7	8	9	10	11	12	*Failures.*
Boys	Failed	1	..	1	..	1	1	1	1	..	..	4
	Succeeded on 1 turn	..	..	..	..	..	..	..	..	..	..	
	2 turns	..	..	2	..	..	..	1	..	..	1	
	3 turns	..	..	1	1	..	..	1	..	..	..	
	4 turns	..	1	2	1	1	2	..	..	..	..	
Girls	Failed	..	..	..	..	..	..	1	2	..	..	5
	Succeeded on 1 turn	..	..	..	..	..	1	1	..	1	..	
	2 turns	..	..	1	..	..	1	..	2	1	..	
	3 turns	..	..	..	..	..	..	..	..	..	..	
	4 turns	1	3	2	2	1	..	..	..	..	1	

The tables merely confirm the low correlation coefficients indicating that in general the child who learns more readily in the Krasnogorski learning is apt to learn more readily to adapt to the changing situation in this test, while the learning is still further conditioned by the ability to unlearn which is essential for adaptation. Or, it is the child who not only learns quickly but who also unlearns quickly that is most apt to do well on this board.

It is interesting to note that all boys who succeeded in completing the first turn likewise did the second turn while the two successes are not always correlated in the girls. On the other hand the girls who do the third turn all do the fourth turn also and this is not true of the boys. Of course this may be only a chance result from so few cases.

Regarding the three Healy Boards very little data were obtained. The boards were not presented as a learning problem in the manner Schmitt used them but as a completion problem or task. As such they interested the children but errors and difficulty usually led them to express a preference for the Seguin Form Board. No successful completion was obtained from any child under the age of 52 months. A girl of that age completed the Foal and Mare Puzzle in 300 seconds but failed on the other two boards. No other child under 70 months of age was successful on the Foal and Mare Puzzle. No child under 55 months of age completed Healy A and no child under 67 months completed Healy B.

On the Foal and Mare Puzzle only ten of the fifteen children who were over the age of 70 months succeeded nor is the failure definitely correlated with any other process tested.

Similarly, eight children completed Healy A in times ranging from 30″ to 325″, and nine, five that did the Healy A and four others did Healy B in times ranging from 90″ to 330″. The failures for children of the same ages, mental ages, etc., are more frequent than the successes, however. Consequently all that we can decide is that success is indicative of an ability not generally possessed by children of these ages but whether failure is significant can not be determined.

All of the cases worked either by chance or by the trial and success method.

The other measurements of the children were of two kinds: the purely anthropometric and the psychomotor. The purely anthropometric measurements consisted of the standing and sitting heights of the child and his weight. The psychomotor measurements were the dynamometric record the child achieved with his left and his right hands used separately and his spirometer ability. These are usually called the "grip" and the "vital" or "lung capacity." These last are measured indirectly through the use of apparatus and consequently measure not the maximum efficiency of the muscles and the lungs as such but the maximum efficiency of the child to voluntarily utilize the capacities inherent in his organism. As this introduces a mental factor they are measures which should probably correlate more highly with each other than with the purely anthropometric findings. This is actually the case.

The height and weight as such are less significant than their relation to other measurements as indicated by indices, hence we have made no detailed study of them separately. The small number of cases would in itself seem sufficient reason for not attaching any significance to them as norms of height or weight, especially as all analysis would have to be by age and sex. In studying mental processes, however, and psychomotor functions, the thing we most desire to obtain is information concerning the relation of these measures to psychomotor and mental growth. If they are at all closely associated with the development of mental ability as tested by the various present-day procedures the relation should appear when the two sexes are studied together.

The following correlations with chronological age were found:

Height, standing	+ 0.898
Height, sitting	+ 0.908
Standing-sitting height index	+ 0.642
Weight	+ 0.894
Weight-height index	+ 0.673
Grip with right hand	+ 0.871
Grip with left hand	+ 0.848
Average grip	+ 0.867
Grip-height index	+ 0.829
Grip-weight index	+ 0.844
Spirometer ability	+ 0.862
Spirometer ability-height index	+ 0.827
Spirometer ability-weight index	+ 0.762

Of these we selected several for further study and correlation. The average grip was chosen instead of either the right- or left-hand grip because it equates the values for right- and left-handed children and since it is the average of the best trial for each hand its value is less apt to be influenced by chance factors. The grip-weight and grip-height indices were also studied because of their possible value as giving indications of physiological age while the spirometer-height and the vital indices were studied for the same purpose.

Average grip and the two grip indices may be considered together. The grip-weight index seems to be slightly more valuable than either of the others as it correlates slightly higher with the results of the mental examinations. The grip indices invariably correlate more highly with the mental ratings than do the vital

indices if we consider the Binet, Yerkes, Adaptation and Form Board tests and the Krasnogorski unlearning while the vital indices correlate significantly higher with the Krasnogorski processes of learning, memory and relearning. This correlation with learning may seem to be partly due to the fact that the use of the spirometer involves more learning than the use of the dynamometer but such can hardly be the case as the children were given similar chance to improve their record on both pieces of apparatus. The best record was used regardless of the fact that it was the first or the last record that the child made.

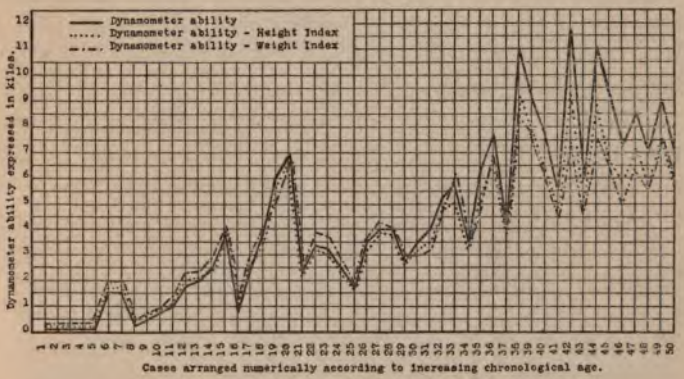
Grip records were obtained upon all of the children over two years of age. The five under that age failed seemingly because of lack of being able to understand what was required of them and not because of their inability to grip anything at all. The same children failed to make any record upon the spirometer with the exception of the one nearest to two years of age (22 months), who inhaled instead of exhaling. The record was used despite this reversal of the normal way of making a record but the other four who were all under sixteen months of age could not even comply with the suggestion to "suck it" or "taste it," which was given in an attempt to get them to make a record by inhalation after they had failed in an attempt to make one by "blowing."

The average grip distribution is seen in Chart V. If we take the average grip of the child and divide it by his height we have the increment of grip per increment of body height. The distribution of this index follows regularly that of the grip itself, showing that the variation in the grip of the different children is due to some factor than that of physiological development as

measured by height, although height regularly influences the grip amount. This independence of the factor of height is indicated by the unusually high correlation coefficient of + 0.989 between grip and the grip-height index. Similarly the influence of weight may be studied by calculating the increment of grip per increment of body weight.

Body-weight seems to have a somewhat greater in-

CHART V. AVERAGE DYNAMOMETER ABILITY AND ITS RELATION TO HEIGHT, WEIGHT AND AGE.


Dynamometer ability
Dynamometer ability - Height Index
Dynamometer ability - Weight Index
Dynamometer ability expressed in Kilos.
Cases arranged numerically according to increasing chronological age.

fluence upon the individual variations than height has, reducing them and bringing them nearer to a group average. Significantly enough it does not seem to raise the relative rating of those whose grip was less than the average as much as it decreases the variation of those above the average. The grip-weight index has in general the highest correlative value of the three when we study their relation to the mental tests. The sex differences do not appear to be very great although the girls seem more variable among themselves while the range of variability is greater for the boys.

The correlations between average grip and the learning and unlearning of the conditioned reflex are rather low but on the whole the more rapidly a child learns or unlearns the better is apt to be his grip or dynamometer ability. There are many individual exceptions to this generality and the relation is again influenced by sex, age and by the high correlation the grip has with things which indicate motor control. Grip or dynamometer ability surely deserves a closer study and we may some day find that its variation when the height and weight factors have been eliminated will be contributive to a better understanding of the relation existing between physiological and mental development.

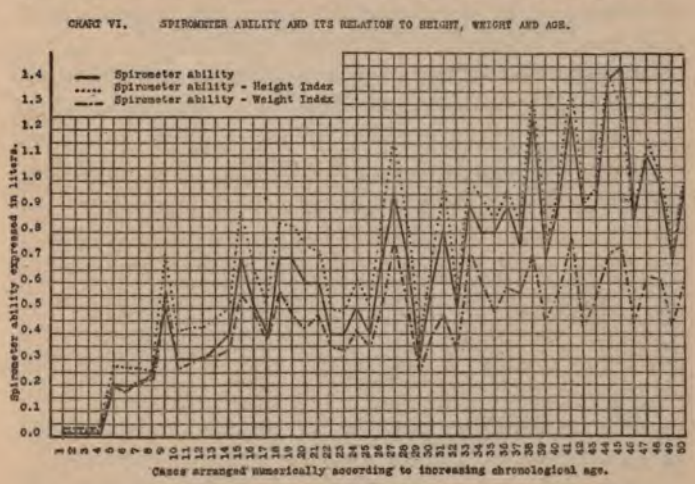
The lung capacity as measured by the spirometer gives a similar distribution to that found by the grip measurements. The spirometer ability-height index or increment of lung capacity as measured upon the spirometer per increment of body height is, like the grip-height index, quite similar to the original measurement when the factor of height had not been eliminated. This can easily be seen in the close approximation of the two curves on Chart VI. The lung capacity-weight index is quite different. The weight seems to be a determining factor in the increase of lung capacity in the children over five years of age. It also influences the variations of individual cases.

The lung capacity-weight or vital index has been rather widely used. DeBusk (36) finds that children who are retarded show a lessened vital index. Since then he has also declared that there is a definite relation between the vital index and mental age according to the Binet although he has presented no correlation coefficient (Unpublished doctorate thesis).

There is a correlation of + 0.739 between the vital

index and the Binet age of the children measured in this study. The correlation coefficient of the lung capacity-height index with Binet age is even higher, being + 0.769. But neither of these is as high as the correlations between grip and Binet rating.

The same is true of the Point Scale or Yerkes ratings. The correlation coefficient with vital index being + 0.679 while that with the lung capacity-height in-



dex it is + 0.722 and with the grip-weight index it is + 0.755.

There are moderate correlations between the vital indices and Form Board and Adaptation scores but in general these are not so high as the correlations of the same scores with the grip indices.

The Krasnogorski learning and the vital index have a correlation of — 0.548 or there is a tendency for the more rapid learners to have a greater spirometer ability-weight index. The correlation coefficient for learn-

ing ability and spirometer ability-height index is almost as high, being — 0.516. On the other hand the correlation coefficients expressing the relations between these indices and unlearning are so low as to be practically negligible.

On the whole the correlation coefficients indicate that both of these indices as well as the grip indices should be used as supplementary tests of mental behavior.

RESULTS OBTAINED UPON DEFECTIVES

In regard to the question as to whether Krasnogorski was right when he claimed that the results obtained by his method have such high clinical value we must turn for a specific answer to our experiments upon mental defectives. The fact, pointed out above, that in our unselected group the children found at the extremes of the age distributions for boys and those found serially misplaced by Binet or Yerkes, when the basis of grouping was the number of trials on the learning process, are the deviates of the group as a whole—this fact shows that we have a method which is able to help determine defectives. Whether it can do so independent of correlation with other findings is another matter.

The work upon the seven lower-grade defectives will, with the exception of cases 6 and 7, throw very little light upon the problem. The rate of learning of the first five cases is as follows:

Case No.	*Age in months.*	*No. of trials for learning*	*Mental Age in Years.*
1	55	14	1—
2	50	5-6	1
3	51	13	1 or 2
4	60	10-19	1[2]
5	36	6-9	1[3]

Two of these establish a reaction within the range of number of trials found upon our unselected group. Their reaction is nearer that which one would expect of their chronological age than that which corresponds to the same mental age in our unselected group. The other three are far slower learners than the youngest normal children tested. But then such a test is not actually needed as all of the five are so definitely idiotic that a superficial examination reveals the hopelessness of the condition to a clinician of ordinary experience. Case seven is somewhat higher grade but again definitely defective. He learns in 8 trials on one process and in 12 on another. The greatest number of trials used by any of those in our unselected group of children who are over 37 months is 7, Case 7 needs 8 and in mentality he belongs about with the three-year-olds who need seven or eight trials, although he is 73 months old. Case six is only 34 months old, but unlike our normals of that age who require about 7 or 8 trials he needs 18 to form an association. One feels by this comparison that the diagnosis of "imbecile" made on him is all he can ever fulfill, although by other tests he is at present practically normal.

The seven children in the Waverley Institution who tested nearest to normal give us more valuable data although the number is unfortunately small. Nevertheless it is an unselected group since all those available under 8 years of age were used, being 6 boys and one girl. The six boys ranged in age from 57 to 93 months. The girl was 81 months old. Three of these tested "at age" on the Binet and were on observation with diagnosis deferred. Of the other four three were one year and one was three years backward by Binet.

The following table shows the condition fairly well:

DISTRIBUTION OF 7 BORDERLINE DEFECTIVES.

Case	Age in months	Binet Age	Point score on Binet	Point score on Yerkes	Krasnogorski Processes: Learning	Recall	Unlearning	Relearning	Institution Diagnosis
A	84	4 1	16	10	3	3	16	2	Imbecile
B	70	4 1	17	9	5	2	8	2	Moron
C	89	6 1	26	25	8	2	15	2	Moron
D	59	3 3	14	5	5	3	11	2	Deferred
E	57	4 3	17	9	6	2	14	2	Deferred
F	93	7 2	32	37	5	2	14	2	Deferred
G (girl)	81	6 1	27	25	13	7	8	2	Probably F. M.

From this we see that with one exception the defective boys learn just about as well as the boys in our unselected group. Indeed, three out of the six learn in five trials which seems to be the mode for the unselected group also. One defective, however, uses eight trials. He is 89 months old and tests 26 points by the Binet. None of our unselected group of children who was over 37 months old or who tested more than 16 points on the Binet was so slow in forming a conditioned reflex. When we add that the same boy needed 15 trials to unlearn the conditioned reflex it is even more obvious that he lies without the normal group.

This difference in the ability to unlearn the conditioned reflex is marked in the majority of cases. Only two of the seven develop unlearning in a number of trials which is small enough to lie within the normal range of variability. The normal range is 3 to 12 trials. One of the defectives develops the unlearning in 8 and one in 11 trials. Both of these are below what they should be on the Binet. This seems more than a chance relation when we consider that of the boys in our unselected group who were of equal age the only ones needing more than 7 trials were the son of the woman with the goitre, who took 8, the defective boy, taking 9, the son from the family with glandu-

lar disturbances using 10, but of these three only the defective boy was below by Binet while five boys between 32 and 51 months of age, normal, with 12-24 points on the Binet, use 8-12 trials.

The one girl tested seems evidently very abnormal—using 13 trials to learn, needing seven to recall and then eight to unlearn. In spite of the fact that she is bright, attractive and playful the experimenter personally feels that these three processes give definite evidence of her mental deficiency as well as of that of the six boys, although they all appeared to deviate but little from normals on the various Form Boards, etc.

Two other children who were available were studied. These were brothers who had been taken to a public clinic for mental examination because they were "not getting along well in school." They were diagnosed as "not feeble-minded." They are 85 and 75 months old respectively. The family is of rather low mentality even for that of an unskilled laborer and the mother's relatives are all of greater ability, tending towards the lower professional class. The mother herself is good natured and easy-going but cleanly and she evidently makes the most of her husband's scanty wages in caring for five growing children. The father seems stupidly intelligent but without any definite virtues or vices. His mother is reported to have suffered from a disease of long-standing which from description could be nothing but syphilis. The three other children, girls, are rather nervous, high-strung and excitable but are not feeble-minded although the one who is in her thirteenth year has begun to fall back from leading her class and has had chorea. The other two, twins, are seemingly normal but similarly neurotic.

The older of the two boys, 85 months old, scored 32

points by Binet, 29 by Yerkes, did the Form Boards readily but if told to hurry became erratic in his movements; was up to the normal averages in his grip and lung capacity. He developed the conditioned reflex in 4 trials, recalled it in 2, developed unlearning in 7 and relearned in 2 trials. These records are all within the normal range. He has had a great deal of illness since early childhood, and is threatened with tuberculosis of which he has all the gross signs. He has a spinal curvature, spay-foot, is chicken-breasted, has a very high palate, is generally anæmic and has had nine peritonsilar operations. This seems sufficient to account for his rather poor school work without implying any absolute mental defect but with imperative indications that the problem is a physical one.

With the brother 75 months of age the case is different. He is and always has been healthy. He is, for his age, unusually large, especially when he is compared with the rest of the family in development. On the Binet he scores 6^2 years or 27 points, while on the Yerkes he scores 25 points. He does the Form Board well, fails on the two harder turns of the Adaptation Board and completely on the Healy Boards. He takes 7 trials to develop the conditioned reflex, recalls it in 2 and then requires 21 trials to unlearn it although he then re-learns it in 2. It is not difficult, keeping these facts in mind, to see why, although he is not feeble-minded by the Binet or Point Scale, he may be actually impossible so far as school improvement goes. School is the prolonged test of social efficiency and ability to adapt and learn which is given all children.

This boy has the ability to form the conditioned reflex or association as readily as normal children but when this simpler situation is complicated by a new

condition he can not adapt readily to the new condition but his old useless reaction hangs on in its initial form. This gives us sufficient insight into his mental-make-up to enable us to see readily why he is a school failure. He can pick up bits of knowledge, form associations, remember impressions, but he can not utilize these experiences in the production of purposive and effective reactions in situations which vary constantly. The reason for his failure lies in his inherent neural predisposition or "set." Because of this he fails on the real mental test of acquiring an education. The experimenter does not doubt in the least but that he will eventually prove defective by other mental tests when sufficient time has elapsed for him to have reached his limit of development. From experience with the other group of borderline cases it seems probable that he will eventually prove to be about of the low-grade moron type.

DISCUSSION AND SUGGESTIONS

The mere execution of a piece of experimenting, such as the writer has tried to carry out, tends to the development in the experimenter of certain "attitudes" towards the problem both as regards subject matter and technique, while usually certain more general points of view develop seemingly unassisted. The statement of these, in so far as they would affect further work by the experimenter, should be considered an essential part of the exposition of the problem.

The first condition of successful work with young children is, in my opinion, a real interest in child life and a knowledge of how to handle children in the thousand and one exigencies of a morning's experience with them. To be able to give drinks of water, wash hands,

tie up bumps, prevent quarrels and return the children to their homes without any signs of physical or mental disturbance is fundamentally a test of ability to handle them under experimental conditions. Women have an undoubted advantage in this field and partly for these reasons. The future of Child Behavior seems to the author intimately bound up with the recognition of this superior fitness of women for such investigations. Needless to say such fitness is only one prerequisite and can not be expected to replace exact training in experimental methods.

And now regarding the children themselves. It is fundamentally necessary to consider their state of health or fatigue, and their emotional attitude towards the experimenter and the experiment as well as to gather the actual results upon the problem in hand. The problem of health can be treated either by eliminating all children who are ill or by noting the condition and observing its effect upon the results obtained. Fatigue is a large and serious problem. In this study all children were used between nine and half-past eleven in the morning, the younger or delicate children on a given day being used first. Fatigue symptoms were never present in any marked degree in the unselected group. Probably the stimulation of the novel situation rendered them less liable to fatigue. It appeared in a marked degree in the child worked with through a number of weeks.

The emotional attitude can usually be kept favorable. It is far better to tell the child that you will allow him to come and play with your playthings than to beg him to be a good boy and come. Most children, and indeed I know of no exceptions, are as delighted to see the queer new things in a laboratory as they

are to go to the circus. Then, when they have come, this interest can easily be maintained by a deliberate presentation of all work as a game, a play, a puzzle or a challenge. The time limit becomes a deadly enemy to be beaten, often objectified as the racing hand of the stop watch. An incredulous "No, I don't believe you can do it again" will be sufficient motivation for a second attempt of a difficult task from which interest departed as soon as it was solved. One little girl of four failed upon the Seguin Form Board and said, "The blocks wouldn't stick." Five minutes' play in which we "made believe" paint them with glue to make them stick led to self-confidence, intense effort and joyous success. In another instance everything done had to be put down on paper in a wonderful system of hieroglyphics so that the successes could be remembered "to tell father." All this takes time but it is more than worth while.

The Aussagen or "free reports" of the children which denote their attitude towards the experiment are well worth jotting down, even though one may make none but a personal use of them. For instance, the attitude of the children towards my experiment was largely one of their attitude towards the feeding. A number of them said "Oh, it's candy," "It's fudge," "It's sweet," when first fed but by far the greater number of these verbal reactions came during the first few trials of the period when I was attempting to develop unlearning. Some of them only smiled sheepishly or looked at me shyly when the bandage was taken off without candy having been given. Several asked, "Is the candy all gone?" "Won't I get any more to-day?" while one little girl offered to go to the store and buy me some more. Another little fellow broke out into an unusual

stutter and said, "You-you-you forgot to give me the candy that time." Another counted, "That's two times I didn't get any, this is three times." Others contented themselves with stating, apparently to the ceiling, that they liked candy. One impudently begged, "Give me just a little bit this time, won't you?" Another stated circumspectly, "I opened my mouth that time" and then on the next trial, "I opened my mouth again" but, when I asked why, she said, "I don't know," although her confusion and shy changing of the conversation indicated that she probably was attempting to give me an indirect intimation of her desire for candy. The insight these reactions give makes interpretation obviously unnecessary.

The number of cases used in this study is far too small to give results which can be accepted as absolute and final and I desire to make no dogmatic assertion of the manner of functioning of the conditioned reflexes in children in general. A group of 50 even with the control groups of defectives is far too small and no one wishes more than I that the number were 5000. I believe that a more extensive study will show that the functioning of the conditioned reflexes is influenced by even more factors than I have been able to indicate. Consequently these results are presented as suggestive and all contradictory evidence is cheerfully invited as that will mean more intensive and extensive study of young children.

The sex differences which I have discussed a number of times in this chapter may be thought to be mere interpretations of insufficient data, introduced because of the writer's personal bias or interest. Quite the reverse is true. All of the first analyses of the findings were made without separating into sex groups. It did

not seem probable that in a group of fifty children all under the age of eight there would be sufficient difference to make it worth while considering boys and girls separately. However, when all other factors had been accounted for there were still irregularities in the distributions which could not be accounted for but these disappeared when as a last resort the two sexes were considered separately.

A point which may be of interest to clinical psychologists is the fact that in every instance the observation of the child during the experiment gave reactions visible to the eye in the same trial as their earliest development showed in the kymograph record. Indeed often the record showed a curve due to cough, a laugh, a shrugging or twisting of the head which could only be understood because the child had been closely watched and because after each trial his general behavior had been written down. Very often the exact situation was more vividly expressed by the general body position, tension and deep breathing than by the mouth opening itself. For clinical purposes one might easily apply the method without any apparatus.

I have made no attempt to evaluate the curves recording the reactions. Krasnogorski explains his curves as representing first a mouth opening and then succeeding that a series of swallowing movements. My results were not so uniform. With some children this type of reaction appeared but with others the mouth would open two or three times with swallowing movements interspersed. In other cases the mouth would be opened wide, gradually closing, and the swallowing curves would be scattered along this decreasing curve. In others breathing was so violent as to give an additional complicating factor in the curves while with

others lip movement (not recorded) was the chief reaction and the swallowing movements were very faint and mouth opening entirely lacking.

The use of so many auxiliary means of studying the child perhaps needs further emphasis. If the study of young children is to lead us to a factually based concept of the child's behavior all of the facts ascertained regarding him must be empirically welded into a synthesis. This can not be done if we completely isolate the study of any one process from that of all others. Hence each study undertaken should involve at least two distinct processes and the results obtained from the two should be correlated. Another experimenter by selecting one of these processes and a new one can thus not only contribute new data but he has a means of correlating all of his work with that of the first investigator.

Another just as important reason is the need of knowing about any one child all the factors which may influence the results. For this purpose social conditions, heredity, sex, age, general health, and as many other items as possible should be considered. Krasnogorski may have considered these but he presents no data. He claims a clinical significance for the conditioned reflex but does not present his criteria. If the cases he used were already defective enough to be diagnosed without this method little benefit accrues from it. Consequently in this study the variety of procedures used has a double purpose.

It makes an attempt to begin a correlative study of the measures usually applied in the study of any child with the aim of placing him relative to his group. It also gives a number of evaluations of each individual child which enables one to more clearly analyze out the

possible factors determining why he holds this or that place in the group. Because of the former need the correlation coefficients of the various tests have been calculated. The second usage is illustrated in the individual case illustrations where an explanation of a varying factor in one process aids in interpreting deviations within the group when it is studied from the aspect of some other process.

This latter point is beautifully illustrated in the case of children in the same family. There are individual conditions as well as the family and social conditions affecting both alike. Differences which remain when age and sex have been evaluated can hardly be attributed to "just individual differences" any more logically than they can to chance errors of experimentation unless we find the same variation in other corroborating findings and these can not be picked up by chance afterwards but must be a part of the original data.

The opportunity of studying children under one year of age was lacking in the village where the unselected group was observed. Consequently it resolves itself into a separate departure which the author has not yet attacked. Krasnogorski states that the conditioned reflex can with difficulty be established in the second half year of life but not until then does it reach its functional perfection. This arbitrary determination of a starting place seems either a conclusion based upon a few failures, probably under unpsychological conditions, or else it may be a logical deduction. Present knowledge of child development would tend to lead instead to a belief that the ability to form some kinds of associations is present in some degree from the time of birth. The development of the association may be more difficult, that is require more trials, and it may

not be possible within the first day or two due to the enormous adaptive changes taking place within the organism. But this very potentiality for adaptation to the new environment is a type of learning and promises interesting results. Moreover, plenty of evidence has been found by observation and in the biographical and genetic studies.

When a child has learned to stop the hungry cry first when he feels the breast, later when he is picked up and still later when he is spoken to this is a conditioned reflex. The touch or sight stimulation is a conditioned stimulus and has been associated with the feeding. Such associations are reported in very young children by Preyer, Stern, Shinn, Mrs. Hall and Dearborn, while the writer observed a conditioned crying in a child 10 days old, fed, warm and dry, who cried until taken up and held and who repeated crying as soon as he was laid down. It was reported that this had happened at the same time each day since the seventh day.

The use of abnormal and borderline cases together with an unselected group seems doubly advantageous to the writer. If a process is fundamentally necessary for normal functioning there should in all probability be greater deviations from the norm among defectives than in the unselected group. The "edges" of the two should, in all probability, overlap. If the process is so generally non-variable that it is the same in defectives as in normals we must evaluate it less highly in the normal child and seek elsewhere for an explanation of why all are not equally gifted.

The question might easily be raised as to why the results are not compared in detail with the results obtained in other studies. For instance, this study in-

dicates a direct relation between learning and effective retention. This is in agreement with the findings of Henderson (76), who used meaningful material. He, also, as I have, found that the older subjects tended to learn more rapidly than the younger ones. Norsworthy (123) also finds the rapid learner retains better and Busemann (24) is inclined to accept this view while Lyon (111) not only confirms this point but finds women and girls do better on initial learning while men and boys retain better. However, he finds many exceptions for "illogical" material. All of these studies as well as those on inhibition have been made on older subjects and have used verbal Aufgaben and chiefly verbal learning material. This study differs not only in the age of subjects but especially in method and material. Consequently minute comparisons are left to the reader interested therein.

The principle underlying experimentation with the conditioned reflex is, however, not as different from that usually employed in the learning process as it appears at the first glance. In the learning process both nonsense syllables and meaningful words are used. These are always grouped and a single task may involve 10, 12 or, it may be hundreds of units. In the study of the conditioned reflex this task is simplified. The child learns only one unit. The stimulus is one that is arbitrarily chosen and is undoubtedly meaningless to the child from the standpoint of our idea of the reaction to be developed. Meaning gradually accrues, that is new associations are formed between the sensations from the conditioned stimulus and other sensations and responses. Because of this simplification of material the determination of retentiveness combines easily the method of saving with that of the determi-

nation of retained members. The stimulus is applied. If the reaction occurs at once the member is retained but if it does not occur the chocolate "help" is given again and relearning is begun. Thus sub-liminal traces are as easily and accurately evaluated as actually retained associations.

The voluntary comments made by the children indicate that the stimulus rapidly develops meaning for them. The meaning appealed to is the most fundamental one of a living, growing organism—food. This appeal probably facilitates learning and it may be that it increases the ease of retention. Consequently a new series of problems opens up at this point. The method probably is the only one we have to-day which offers a chance to study meaning and affective values in young children. The relative values of food eaten, seen, touched, mentioned; of pleasant, unpleasant and new stimuli; of commendation and actual reward may easily be studied simply by varying the stimulus quality, intensity or modality.

And now a few words more about the more general uses of the method. It is easily applicable with very young children. It is independent of the acquisition of speech and hence enables one to study the development of mental processes without considering the language factor. It can however be linked up with speech studies in various ways. For instance, an auditory stimulus may be used as the conditioning stimulus. This may just as easily be a nonsense syllable as the ringing of a metronome. It might even be possible to develop discrimination of nonsense syllables by using feeding when one is said and not with the others. The relative value of auditory, visual, cutaneous, motor and thermal stimulations may be studied by comparing the

length of time necessary to develop conditional reflexes to each of these. Naturally this must be done in one and the same child and as there may be a transfer of attitude or attention from one series to another the series must be studied in various children in different orders of development. Sensory discrimination and specificity and memorial functioning of stimuli of various modalities may also be studied and may throw some light upon the better modalities of presentation for best learning and retention. The verbal Aufgabe may be introduced and its effect show relatively. Both the negative and positive Aufgabe may be studied thus.

So far as an investigation of the factors influencing the learning process is concerned this method allows of such wide variation that time factors, effect of frequency and distribution of stimulations, of intensity and complexity of stimuli, of modes of presentation, quality of stimuli, of effect of affective toning, near and distance stimulation may all be studied while the child thinks he is playing a game.

CHAPTER VIII

CONCLUSIONS

I HAVE tried to present briefly the facts which a survey of child study gives, disregarding details but stressing in a broader more synthetic fashion the factors influencing its development.

Child study has been the outcome of a new consciousness in the race and society which has focused upon the child as its most valuable asset. The first studies made of the young child were motivated primarily by ethical, moral, religious and educational aims. Not the child's abilities or unfolding mental processes as such were the object of study but they were regarded as means of giving him training and education. The study of the child as such, for his own sake, was not properly emphasized by any one before Schleiermacher. Even after his formulation of childhood for the sake of the child it took many years, a quarter of a century in fact, before we find any expression of interest in the child's person, his body and mind, motivated by a purely scientific desire for a better knowledge of the child himself.

Then in the work of Sigismund, Kussmaul, and their followers comes the beginning of careful observations of child nature. These studies do not, however, issue from psychological or educational circles but are the work primarily of physicians. Physiological methods and theories were sufficiently well evolved to permit in-

vestigation of physical functions. Psychology had not yet developed far enough to have even a method that might be used for any such studies.

As psychology gradually devised its experimental methods these were based largely upon introspection and the study of the young child seemed consequently limited to observation. This was considered a handicap and for some time undoubtedly proved a rather effective deterrent to quantitative experimental studies under controlled conditions. Nor had these as yet evolved their independent procedures. This tendency to observational study of children was also favored by the difficulty of gaining permission to study any large number of children during the years before the kindergarten age. In this early period the social group of the child is that of the family and he is not segregated with a large number of his kind unless he is in an abnormal physical or mental condition and committed to a hospital or institution.

With the added impetus and suggestion given by the popular interest in evolutionary theory, genetic or developmental studies were vigorously and assiduously compiled. The external expressions of the child through language were also studied both from a genetic standpoint because of the interest in his development of the language function and also from the standpoint of indirect observation because it was assumed that his use of language gave a fair index of his mental processes. For the first purpose the studies were usually intensive and of only one or two children and mostly made during the first three years of life while for the latter they tended to become contributive to education and the subjects were usually children of five or six years of age who were just entering school.

The intermediate period of two or three years remained almost entirely unexplored.

The child's movements were also used as a means of studying his mental processes. For instance: grasping was made a means of studying color discrimination and color preference. These studies most nearly approximate the requirements of scientific experimentation, allowing of control of conditions and giving easily evaluated results but their use seems limited to a very narrow field.

Individual studies have not been confined to these fields of physiological psychology, language and motor development but have touched almost all subjects pertinent to psychology. As a whole, however, either the number of children used has been very small, the method faulty or the findings so mixed up with inference and discussion that the results are equivocal from the standpoint of their empirical value. The psychogenetic or biographical studies were and are valuable. They have been the pioneer path-breakers that indicate the extent of the problem confronting us but now exact definitization of facts demands quantitative studies under controlled experimental conditions and an unbiased presentation of the results obtained.

Some such experimental investigations have lately been made by those interested in Comparative Psychology. They have used various of the objective methods upon children with rather favorable results. In so far as they have been interested in animal rather than human psychology their work shows unmistakable errors. Although making comparisons of the results obtained upon children and other animals they introduce great variants in procedure when dealing with the two. The verbal Aufgabe with children takes the

place of objective sense stimuli used with the animals or if objective stimuli are given they are such as appeal differently in the two cases. The number of children, with the exception of Katz' studies, is very small. In general, the reports show that the experimenters were working with inadequate and often erroneous ideas of what the child as a type means.

This attempted use of purely objective methods in the study of the child's behavior is a decided advance, however, for errors in technique can easily be remedied and, although most of the methods used so far have been rather unwieldy and cumbersome, we can see now the theoretical and objective basis from the use of which the experimental psychology of the young child will rise. This new attitude towards the child conceives of him as an organism peculiarly adapted to growth and functional responses to the external stimuli among which he lives. Our information concerning this organism, its peculiarities and abilities, is best increased by studying the variations of its behavior under controlled or definitely ascertained conditions of stimulation. These variations may be changes in behavior of growth and structure formation or they may be changes in function. The study of the latter changes in so far as they relate to or are neural in character is peculiarly the property of psychology. These changes are not, however, to be thought of as existing independently of or uninfluenced by the characteristics of the organism considered as a whole.

The chief impetus towards the formulation of this concept of the psychology of the infant has come from the Pavlov school, especially as it has been applied to human beings by Bechterew and Krasnogorski. The work of Bechterew, followed by that of Watson in

this country, is less significant for the study of the young child than is that of Krasnogorski. Most of Bechterew's work has been developed upon the punishment motivation to protective response. In children this induces attitudes towards the situation which are absolutely antagonistic to the purpose we have in mind of studying the child's reactions under normal conditions.

Krasnogorski more nearly fulfills the demands our concept of the child makes upon us. His assertions of the value of the method of conditioned reflexes as a means of studying young children pointed to a further study of the method itself as necessary and probably valuable. His work is, however, distinctly medical and the terminology needed interpretation and assimilation into that of our psychological studies while his method needed refining and standardization.

A careful study of the terminology shows that the conditioned reflex is neither more nor less than an association developed between an arbitrarily determined stimulus and some response habitually caused by some other stimulus. This response may be one functioning in everyday life, or as used by Bechterew, is itself sometimes an artificially developed response.

Experimentation with the method verifies Krasnogorski's claims for it. It can easily be adapted to varying and multiform conditions.

The conditioned reflex can easily be established in normal children of one year of age and in defectives mentally much less. The lower age limit of its possible development in normals has not yet been ascertained.

The results obtained from the use of this method upon a total of 67 children are as follows:

Unselected group.

1. The learning of a conditioned reflex requires from 3 to 9 trials in normal children. This is a narrower range than that found by Krasnogorski.

2. The number of trials required decreases as age increases up to the age of 60 months, above this age the results are less regular.

3. The number of trials required is also influenced by sex. Under two years of age the boys learn more rapidly than girls of the same age. Above two this order is reversed. The number of trials required varies more among the girls than with the boys.

4. With children of the same age those who learn more rapidly are also brighter as measured by the results obtained through the use of the Binet and Yerkes scales.

5. With children learning in the same number of trials the older ones have higher ratings on the Binet and Yerkes scales.

6. In general there is a slight tendency for the children who are more rapid in learning the association to be more rapid in performance of the Seguin Form Board, although when we study the order of merit for Form Board ability we find the most rapid learners on the Krasnogorski method are medium in their ability there.

7. Those who are more rapid in learning are also more apt to succeed upon the Adaptation Board. In general the number of trials completed upon the Adaptation Board is in inverse order when compared with the number of trials for learning, the distribution of the boys being more regular.

8. The number of trials needed for learning decreases as the grip ability increases. This holds in

lesser degree for the grip-height index and most markedly in the grip-weight index.

9. The rate of learning increases as the spirometer ability increases. This is also true of the vital indices.

10. After a twenty-four-hour interval the conditioned reflex functions at once in over seventy per cent of the unselected group.

11. The number of trials needed for relearning varies only from 2 to 7, being less than the learning range.

12. This variability in retention appears independent of the age of the child.

13. There is a moderate relation between learning ability and retention. The most rapid learners retain best and their average score, which without exception was one of complete retention, is equaled by only one other group, the one which learned most slowly. Those intermediate in learning ability are less successful in retaining.

14. The range of trials required for relearning is less among the girls than among the boys.

15. The mechanism of inhibition or unlearning of the association was developed in 41 of the unselected group. The youngest nine reacted antagonistically to the situation which confronted them in the attempt to develop the unlearning and hence scored failures on this process.

16. The number of trials needed for developing the inhibition or unlearning ranged from 3 to 12, the range being greater for the boys than for the girls.

17. In general the number of trials needed for unlearning the conditioned reflex decreases as age increases.

18. There are marked sex differences, more of the boys developing the unlearning rapidly, more of the girls requiring a greater number of trials.

19. The number of trials needed for unlearning tends to decrease as the number of trials needed for learning increases. This relation is more constant among the boys than among the girls.

20. There is a slight indication that the number of trials needed for effecting unlearning is greater the fewer the number of trials needed for memorial re-functioning or re-learning after a twenty-four-hour interval. (This would be in accordance with Jost's law.)

21. For children of the same age the number of trials needed for developing unlearning is in all but one instance greater for the girls than for the boys.

22. For all the children developing unlearning in the same number of trials the mental age by Binet increases as the chronological age increases when subdivision is made according to sex. The regularity of distribution by the Yerkes age is slightly less.

23. In the children of any one chronological age, grouped according to the number of trials they required to develop an inhibition or unlearn the conditioned reflex, the situation is more complex. For boys the mental age by Binet or Yerkes increases, up to a certain point, as the number of trials required increases, then for those requiring more trials the mental age is less. This relation is different in children of different ages. For girls the distribution indicates that the duller children require more nearly the median number of trials, the brighter children developing an inhibition in either a lesser or a greater number of trials.

24. There is a slight indication that those children who are more rapid in unlearning can complete the Seguin Form Board with better time records than those who unlearn slowly.

25. The number of turns of the Adaptation Board which are successfully completed is greater among those children who unlearn more rapidly.

26. There appears to be a slight positive relation between the increase in average grip and the decrease in number of trials necessary to develop unlearning. This relation is less marked when we use the grip indices.

27. There is practically no relation between the vital indices and the number of trials necessary to effect inhibition or unlearning of the conditioned reflexes.

28. The relearning of or re-establishment of the conditioned reflex functions without much perceptible difference in children of all ages and both sexes, although there are several individual variants. The significance of these can not be established without further work at this point.

Defectives.

29. The learning process in defectives is such that they require anywhere from 3 to 18 trials to develop a conditional reflex.

30. The number of trials needed for this learning is not directly proportional to the mental ability of the child but is also relative to his chronological age.

31. The range of number of trials needed to develop a reaction after 24 hours is the same as that in the unselected group, 2-7. However, instead of over 70% using only 2 trials we have here but 4 out of 7 or 57% with whom 2 trials were sufficient.

32. Although only two of the defectives studied were girls there is an indication that with this group as well as with the unselected group there are important sex differences.

33. The number of trials required for inhibition or unlearning varies from 8 to 21.

34. The association or conditioned reflex is in every instance relearned or redeveloped in two trials.

35. The greatest deviation of these borderline defectives as a group from the unselected group lies in the number of trials necessary for the development of unlearning. Only three of the defective group effect this in a number of trials which lies within the range of normal performance and this overlapping is true only when the two groups are treated as wholes. If we place any defective girl or boy in the unselected group in the place where he would belong by virtue of his age, sex and mentality as measured by the Binet or Yerkes scale he will be outside the range of variations of the unselected group in the number of trials he needs to develop unlearning. The only case in the unselected group with which any one from the defective group is homogeneous in behavior in unlearning is the little son of the feeble-minded man and the insane woman.

36. In the case of children whom other tests leave undiagnosed or unprognosed these methods indicate a procedure which makes a more fundamental test of the mental processes and consequently they discriminate potential defectives and psychasthenics in a way that is more highly prognostic than are the results of any other test now employed.

37. The method seems even more valuable because its findings upon any one child seem to indicate that

the conditioned reflex processes are influenced in the rate of their functioning by sex, age, heredity; are correlated with ability upon other tests, with measurements like grip and lung capacity and are probably influenced by glandular disturbances. As a result they give us in a simple numerical form a rating which facilitates our comprehension of the total endowment Nature has given the child.

By no means, however, do I wish it to be thought that I favor the use of the Krasnogorski processes in place of other methods in clinical examination. Instead I believe that the study of the individual child for the purpose of prognosis or merely for a better understanding of his nature is not to be accomplished by the esoteric usage of any individual method, no matter how high its claims. Just as binocular vision gives us a third dimension so every additional line of approach to a child's mental processes allows us to understand and evaluate them more fully.

No child should be diagnosed as normal or abnormal unless he has been studied from every possible aspect. This idea has been emphasized by Fernald (44). Every child must be surveyed through the eyeglasses of anthropometrical measurements, heredity, his history from the time of conception, his educational history, present and functioning educational acquisitions, his general knowledge gained from experience in his social group outside school, his social, moral and economic reactions as well as from the viewpoint of exact measurement of mental processes. Of course the pre-school child is too young to have much of a history in several of these lines and hence it is even more important that the others be accurately evaluated. I do think, however, that experiments such as the Krasnogorski which

deal with definite neutral situations are more fundamental and as they become more numerous will tend to replace the arbitrary tests of acquired knowledge.

But one thing needs to be kept in mind. The child is an individual and must be treated and studied as such whether he as an individual is the whole problem or only one of a group forming the subject matter of a problem. No success may be ascribed to studies which disregard this fact. The day for the mere abstraction of one item, its cold manipulation and the absolute deduction of facts therefrom is past. We must realize the eternal interplay of all of the many factors in the child and his environment. The more of these we can grasp the nearer shall we come to the explanation and understanding of any one of them. This does not mean not submitting the child to arbitrary laboratory conditions for studying him. Give him if you will time limits within which he must do so and so or must not do so and so, but keep the conditions constant for that problem, noting as many factors in the situation as you yourself are aware of. Only by doing this shall we come to an understanding of the whys and the wherefores which make of the child a growing and functioning unity.

CURVES ILLUSTRATING THE DEVELOPMENT OF CONDITIONED REFLEXES

In all curves the dash in the topmost line represents the moment of feeding.

The second line is the record of the throat and chin movements of the child.

The bottom line is the time recorded in seconds.

Case 31.—Gertrude, aged 61 months, who tests 6^2 by the Binet and 5.0 by the Point.

TRIAL 1.—Child lying quiet until fed.

TRIAL 2.—Child not reacting before she was fed.

TRIAL 3.—Mouth opened before feeding.

TRIAL 4.—Mouth again opened before the feeding.

Case 31, after 24 hours.

TRIAL 1.—Reaction before feeding.

TRIAL 2.—Reaction before feeding, child not fed but unlearning begun.

TRIAL 3.—Reaction to bandage, no feeding.

TRIAL 4.—Reaction to bandage, no feeding.

TRIAL 5.—Marked and violent reaction.

TRIAL 6.—Continued reaction.

TRIAL 7.—Reaction continued.

TRIAL 8.—Almost quiet.

TRIAL 9.—Slight swallowing.

TRIAL 10.—Violent mouth opening and swallowing.

TRIAL 11.—No reaction throughout the period.

TRIAL 12.—Child quiet until fed. Unlearning was perfected and re-learning begun.

TRIAL 13.—Re-learning effected. Child opens her mouth before time for feeding.

SELECTED RECORDS FROM OTHER CASES

ILLUSTRATING VARIOUS STAGES IN THE DEVELOPMENT OF CONDITIONED REFLEXES

CASE 30, Margaret, 59 months old, Binet 6^4. Learning was developed in 4 trials, and function was immediate after 24 hours. This is the reaction upon the sixth trial for effecting unlearning. Child lay tense with heavy breathing and opened mouth. Two more trials were needed for unlearning.

CASE 39, Clarence, 73 months old, Binet 6^3. Learning required 6 trials and recall after 24 hours was immediate. Accompanying curve shows unlearning fully developed and re-learning begun after only 4 trials.

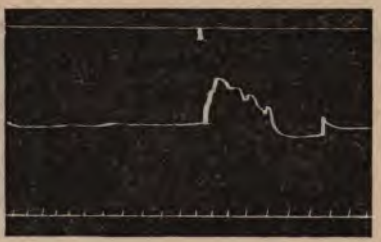
CASE 23, Jackie, 51 months old, Binet 5^4. Curve shows learning fully developed and marked reaction before feeding on this, the third trial. In this case unlearning required 9 trials.

CASE 24, Marie, 51 months old, Binet 5^2. Learning required 5 trials, recall was immediate but unlearning took 10 trials. Re-learning was then begun. This curve shows the third trial for re-learning which evidenced itself on the fourth trial. No reaction before feeding.

CASE 19, Russell, 48 months old, Binet 5^3. Learning complete on this the fifth trial. Recall after 24 hours required 3 trials and unlearning 7 but re-learning only 2 trials.

CASE 12, James, 36 months old, Binet 3^4. After forming the association in 7 trials he recalls in 2 trials and then takes 12 trials to unlearn. The curves show the ninth and eleventh trials in the development of unlearning.

CASE 12, Ray, 34 months old, Binet 4^1. Learning took 6 trials and recall 3 trials while unlearning took 6 trials. The sixth trial for unlearning and the first feeding for relearning are shown in this curve.

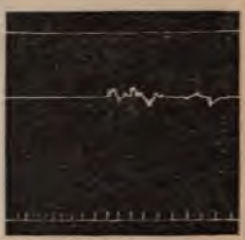


CASE 25, Alice, 51 months old, Binet 4^3. Learning took 3 trials and recall was immediate after 24 hours. Unlearning took 8 and re-learning 2 trials. The curve shows the fourth trial at unlearning, given after she had said, "Give me a piece of candy this time." "Psychic stimulation" is surely at work.

CASE G, Esther, 81 months old, Binet 6^1. She developed the association in 13 trials. Its appearance is shown in curves for the eleventh and twelfth trials reproduced here. She took 7 trials for the 24 hour recall, 8 to unlearn and 2 to relearn.

Case F, Crahil, 93 months old, Binet 7^3. He learned in 5 trials and recalled in 2 but took 14 to unlearn. Curves 12 and 13 are presented here and show the disappearance of the reaction. He relearned in 2 trials.

Henry, 75 months old, Binet 6^2. This is the boy who could not get along in school but who by other tests is apparently normal. The curves show the nineteenth and twenty-first trials given in an attempt to have him unlearn the reaction learned in 7 trials. He reacts in the nineteenth but has by the twenty-first been quiet twice successively and re-learning is begun.



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